

THE PSYCHOLOGICAL REVIEW.

VISION WITHOUT INVERSION OF THE RETINAL IMAGE.

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In the November number of this REVIEW, I gave a short account of some preliminary experiments on vision without inversion of the retinal image. Brief as the experiments were, they gave certain definite results and hinted at others which would probably be obtained if the artificial conditions were continued for a longer time. The course of the experience also showed that problems much wider than that of upright vision were involved, and that a careful record of a longer test might throw light on these also. I was strengthened in this view that the experiment bore on other problems at least as important as that of upright vision, by the remarks of Professor Titchener when the paper was publicly read; while the questions of Professor Münsterberg, on the same occasion, suggested the need of more careful observations in regard to dizziness and the localization of sounds.⁽¹⁾

The earlier paper was thus necessarily vague or silent on a number of questions in regard to which a more careful and extended experiment could hardly fail to produce something of interest—on such questions as, for instance, whether the reconstruction of the directions, right and left, proceeded exactly parallel to that of the directions up and down; what the connection of visual and tactual localization really is, which enables the one

¹ See the *Berichte* of the Third International Congress for Psychology. Munich, 1897, p. 194.

to influence the other ; and, finally, what were the more definite conditions under which the harmonious accommodation to the abnormal sight-perceptions waxed and waned. It was also necessary that a nicer distinction should constantly be observed between acts or ideas arising as a result of deliberate volition and those which arose effortless and unpremeditated. In other words, the account should clearly distinguish at any given stage of the experiment between processes which occurred spontaneously and those which could be called up only by force of will.

The present experiment was conducted under almost the same conditions as those of the preliminary experiment. I myself was again the observer, and the apparatus was the one described in the earlier article, except that a thin cloth-lined plaster cast of the region about the eyes was substituted for the padded paste-board case which before had held the tube of lenses. In making the cast a small mass of non-adhesive material was placed directly over each eye, and afterwards removed from the cast ; so that during the experiment the inner lining of the case did not press on the eyes, nor interfere in the least with their free movement. In front of the right eye there was an opening in the cast, into which the tube of four lenses before described fitted exactly. This tube was carefully focussed and set at such a distance from the eye as to give a clear visual field of about 45° compass. The cast could then be bound to the head by a set of tapes, and although somewhat heavier than the paste-board case, was nevertheless much more comfortable, because it pressed evenly over a large surface of the face. By this device all light was excluded, except such as came through the lenses into the right eye.

The time was not spent, as before, entirely indoors. Besides the free range of the house, I could walk in a secluded garden ; and since the experiment fell at a time of bright moonlight, I took, every evening but the first, a long walk through the village, accompanied and, when there was need, guided by a companion. The experiment lasted, this time, from noon of the first day until noon of the eighth day—a net period in all (after subtracting the time during which the eyes were blindfolded),

of about 87 hours, as against $21\frac{1}{2}$ for the previous experiment. The actual record for the eight days is as follows:

DAY.	HOURL OF PUTTING GLASSES ON.	HOURL OF TAKING GLASSES OFF.	LENGTH OF TIME GLASSES WERE WORN.
1st	12 m.	9 p. m.	9 hrs.
2d	9 a. m.	9 p. m.	12 hrs.
3d	9 a. m.	9 p. m.	12 hrs.
4th	9 a. m.	9:45 p. m.	12 hrs., 45 mins.
5th	9:50 a. m.	10:30 p. m.	12 hrs., 40 mins.
6th	9:50 a. m.	9:45 p. m.	11 hrs., 55 mins.
7th	9:15 a. m.	9:45 p. m.	12 hrs., 30 mins.
8th	8 a. m.	12:10 p. m.	4 hrs., 10 mins.
			Total, 87 hrs.

At all times when the glasses were not worn, the eyes were thoroughly blindfolded. Careful notes were made every day, to record as exactly as possible the actual state of the experience at that time.

Before I attempt a narrative of the experience under the experimental conditions, a word or two as to the terminology will be necessary. One has constantly to make a distinction between the appearance of an object as seen through the reversing lenses, and either the appearance it had before the lenses were put on, or the appearance it would have had if the lenses were removed and normal vision restored. This appearance just described is called in the narrative the 'older,' the 'normal,' often the 'pre-experimental' appearance of the object; while the appearance through the lenses is called its 'newer' or 'later' appearance. Similar distinguishing terms have also to be used with reference to the mere representation or idea of an object, as contrasted with its actual perception.

It is perhaps unnecessary to state that the accommodation to the artificial conditions was, in my case, probably more rapid than it would have been, had I not retained some of the effects of the practice gained in the earlier experiment, about five months before.

The experience from day to day was as follows:

First Day.—The entire scene appeared upside down. When I moved my head or body so that my sight swept over the scene, the movement was not felt to be solely in the observer,

as in normal vision, but was referred both to the observer and to objects beyond. The visual picture seemed to move through the field of view faster than the accompanying movement of my body, although in the same direction. It did not feel as if I were visually ranging over a set of motionless objects, but the whole field of things swept and swung before my eyes.

Almost all movements performed under the direct guidance of sight were laborious and embarrassed. Inappropriate movements were constantly made; for instance, in order to move my hand from a place in the visual field to some other place which I had selected, the muscular contraction which would have accomplished this if the normal visual arrangement had existed, now carried my hand to an entirely different place. The movement was then checked, started off in another direction, and finally, by a series of approximations and corrections, brought to the chosen point. At table the simplest acts of serving myself had to be cautiously worked out. The wrong hand was constantly used to seize anything that lay to one side. In pouring some milk into a glass, I must by careful trial and correction bring the surface of the milk to the spout of the pitcher, and then see to it that the surface of the milk in the glass remained everywhere equally distant from the glass's rim.

The unusual strain of attention in these cases, and the difficulty of finally getting a movement to its goal, made all but the simplest movements extremely fatiguing. The observer was thus tempted to omit all those which required nice guidance, or which included a series of changes or of rapid adaptations to untried visual circumstances. Relief was sometimes sought by shutting out of consideration the actual visual data, and by depending solely on tactual or motor perception and on the older visual representations suggested by these. But for the most part this tendency was resisted, and movements were performed with full attention to what was visually before me. Even then, I was frequently aware that the opposite, the merely represented, arrangement was serving as a secondary guide along with the actual sight perceptions, and that now the one factor and now the other came to the foreground and was put in control. In order to write my notes, the

formation of the letters and words had to be left to automatic muscular sequence, using sight only as a guide to the general position and direction on my paper. When hesitation occurred in my writing, as it often did, there was no resort but to picture the next stroke or two in pre-experimental terms, and when the movement was once under way, control it visually as little as possible.

The scene before me was often reconstructed in the form it would have had in normal vision; and yet this translation was not carried to such an extent as at the beginning of the first experiment. The scene was now accepted more as it was immediately presented. Objects of sight had more reality in them—had more the character of ‘things,’ and less that of phantasms—than when the earlier trial began. Objects were, however, taken more or less isolatedly; so that inappropriateness of place with reference to other objects even in the same visual field was often, in the general upheaval of the experience, passed by unnoticed. I sat for some time watching a blazing open fire, without seeing that one of the logs had rolled far out on the hearth and was filling the room with smoke. Not until I caught the odor of the smoke, and cast about for the cause, did I notice what had occurred.

Similarly, the actual visual field was, for the most part, taken by itself and not supplemented, as in normal vision, by a system of objects gathered and held from the preceding visual experience. Sporadic cases occurred, in which some object out of sight was represented as it had just been seen; but in general all things not actually in view returned to their older arrangement and were represented, if at all, as in normal sight. Usually this was the case also in picturing an unseen movement of some part of my body. At times, however, both the normal and the later representation of the moving part spontaneously arose in the mind, like an object and its mirrored reflection. But such cases occurred only when actual sight had just before revived the later memory-image.

As regards the parts of the body, their pre-experimental representation often invaded the region directly in sight. Arms and legs in full view were given a double position. Beside the

position and relation in which they were actually seen, there was always in the mental background, in intimate connection with muscular and tactual sensations, the older representation of these parts. As soon as my eyes were closed or directed elsewhere, this older representation gathered strength and was the dominant image. But other objects did not usually have this double localization while I looked at them, unless non-visual sensations came from the objects. Touch, temperature, or sounds, brought up a visual image of the source in pre-experimental form.

Anticipations of contact from bodies seen to be approaching, arose as if particular places and directions in the visual field had the same meaning as in normal experience. When one side of my body approached an object in view, the actual feeling of contact came from the side opposite to that from which I had expected it. And likewise in passing under a hanging lamp, the lamp, in moving toward what in normal experience had been the lower part of the visual field, produced a distinct anticipatory shrinking in the region of the chin and neck, although the light really hung several inches above the top of my head.

Whether as a result of the embarrassment under which nearly all visually guided movements were performed, or as a consequence of the swinging of the scene, described above, there were signs of nervous disturbance, of which perhaps the most marked was a feeling of depression in the upper abdominal region, akin to mild nausea. This disappeared, however, toward evening; so that by half-past seven it was no longer perceptible.

Second Day.—This feeling of nervous depression, just mentioned, returned the next forenoon. Movements, though, had in many respects grown less laborious, and were performed more on the basis of the actual sight-experiences, and less by excluding these as a means of guidance. Once at least, in the afternoon, I noticed that in washing my hands I had given myself up completely to the actual scene; but at the next instant inappropriate movements occurred, and with the consciousness that I had thus given myself up the old pre-experimental translation of things returned.

Unseen objects could, by force of will, be represented in harmony with things in view, more easily than on the preceding day. I could, for instance, voluntarily bring before me, in consistent relation to the visual field, the general outline of the room in which I was sitting. My own body, however, was much less tractable; at best I could get only my legs and arms appropriately represented, and this only by an effort not required by other objects. And even an unseen object of this latter sort, when felt in intimate connection with some part of the body which stubbornly held its old ground, could not by effort of will be vividly represented in terms of the newer sight.

There was much evidence of a rigid interconnection of experiences, by which the place or reality of one thing decided the place or reality of something else. The vividness with which a part of the body could be localized by visual representation, was influenced to some extent by the consistency of this representation with the actual perceptions of sight. Thus in swinging my clasped hands above my head, although I was aware of the direction of such a movement in the pre-experimental visual field, yet the actual disappearance of my hands *below* the lower border of the field, and the free continuance there of the movement, involuntarily made the region seem, for the time, visually vague and empty where I had hitherto represented my chest and shoulders. Likewise, in walking through the room, the disappearance of a low-hanging electric globe toward the space in which my chin and neck were represented, and the immediately following contact of the globe with the top of my head, tended to disturb the place of representation of both my chin and scalp; while attention to the ceiling disappearing, as I walked along, in what was normally the lower part of the visual field, weakened the connection of the image of my feet with this place in the field. There was thus a suggestion of more than one way of appropriately knitting some item into the body of experience. This not infrequently led to two representations of a single thing, both of which had a sort of reality; although not to such an extent as to give an actual illusion of two objects where there was really only one. The unseen fire-place in the room where I was sitting could be viv-

idly represented according to the new set of visual relations, but the crackling of the fire was involuntarily referred to another direction, and in that direction there was also a dim image of the fire-place. And even when I looked directly at some part of my body, there was an acceptance of the seen thing as the real thing, and yet there was an accompanying transposed representation of it which also possessed a certain reality of its own.

As to the uprightness or inversion of things, the general feeling was that the seen room was upside down; the body of the observer, represented in pre-experimental terms, was felt as standard and as having an upright position. But different circumstances produced a different shade of feeling. When I looked out over a wide landscape, the position in which I felt my body to be and the position of the scene before me were surely discordant and unnatural. Yet I could not, as I had the day before, take either the one or the other unreservedly as standard. It seemed as if an abnormal position of my body in viewing things might just as well account for the facts as would an inversion of the scene. The very expanse of the landscape in comparison with the size of my body no doubt tended to subordinate the latter and render it less unreservedly a norm for judging of correctness of position. But even when, indoors, the view was almost completely filled with the dining-table and its furnishings, there was no striking and obvious feeling that the scene was upside down.

During a rather long walk in the evening I was unable to recognize my surroundings most of the time, although normally they were quite familiar. Recognition evidently depended largely on external relations of position and direction, and, with a disturbance of these, the objects themselves seemed strange. I could voluntarily feel my feet strike on the ground seen in the upper part of the visual field. But my companion, who held my arm, I could not represent on that side of me which, I knew, a harmonious construction of the visual field would require.

On being blindfolded for the night, there was an immediate and involuntary recurrence to the older way of picturing things. Only rarely could anything be represented in terms of the later sight.

Third Day.—I was now beginning to feel more at home in the new experience. At no time during the day did any signs of nervous distress appear, and the hours passed more rapidly than on either of the preceding days.

Walking through the narrow spaces between pieces of furniture required much less care than hitherto. I could watch my hands as I wrote, without hesitating or becoming embarrassed thereby. Yet I often stretched out the wrong hand to grasp a visible object lying to one side; right and left were felt to be by far the most persistently troublesome relations when it came to translating visual into tactual or motor localization. An involuntary feeling of dissatisfaction with the new visual perceptions in some cases produced movements which, although intended to be corrective, were really the contrary. For instance, while holding my hands in water running from the customary faucet, in the wash-bowl, I had repeatedly to suppress involuntary movements of the hands toward the wrong faucet which now occupied a visual position identical with that formerly held by the right one in the normal experience. The visual hands were not in the visual place approved of by the older experience; spontaneous efforts to rectify the misplacement followed, although the motor perceptions were entirely appropriate to the scene, had this been translated into pre-experimental terms. The corrective movements were therefore evidence that a translative reconstruction of the scene had not taken place. And yet the older criteria of inappropriateness of visual position were still active in the new experience. Instead of a reconstruction or translation of this new experience into terms of the old, I now occasionally became aware of an opposite process—a spontaneous translation of some pre-experimental memory-image into the form of the later vision.

Head-movements were still accompanied by a slight swinging of the scene, although in a markedly less degree than on the first day. The movement was referred more to the observer, so that it seemed to be more a moving survey of stationary objects.

It is difficult to describe my attitude of mind toward the inverted scene. Little more can be said than that there was clearly an abnormal relation between the general localization of

my body and the position of the scene as a whole; but, as when looking at the landscape the day before, it was not clear which of the terms was standard and normal and which was thereby condemned. I had, however, a distinct consciousness that the feelings connected with certain positions in the visual field were by no means what they had been in the normal experience. What had been the old 'upper' position in the field was beginning to have much of the feeling formerly connected with the old 'lower' position, and *vice versa*. Once as I stood before the fire-place, watching the fire, an odd sensation came over me, as if I were looking at the fire out of the back of my head.

— Contacts in walking past objects had hitherto for the most part been surprising, because the contact was felt in a different place from the one anticipated. But to-day I noticed that expectation was coming more into harmony with the actual experience. It was also evident that this expectation, when joined with a vivid representation of the region of the body in question, had a perceptible influence upon the direction in which the contact was actually felt. If, for example, I walked up to a low railing which came against my abdomen, the sensations of pressure seemed to come from the new visual position of the abdomen if I called up a vivid image of this part of my body in its new position and expected the sensations to come from there. But the unexpected contact of the railing with my arms (then out of sight), which had not been represented in their new position, was referred only in the old way, until these too were distinctly imaged as the abdomen had been. But even when the localization was in accord with the new visual experience, there was still a subordinate, background localization after the old manner.

Other factors besides volition or even fecency of visual perception were observed to have an effect on the direction in which unseen objects were represented. The position of the shadow of my body in the visual field, for instance, involuntarily strengthened the new representation of my body. Shadows had also a marked influence in determining where I must think the window or the sun to be. And movements of my hands in front of my eyes to some part of my body which I could not see, gave the clue to the new visual position of the part.

✓ In this way and from other influences, there was coming to be a more vital connection between my actual perceptions and the larger visual system of merely represented objects. It was becoming easier to follow a line in the field of sight and, continuing the line into this larger system of things, to know what it would lead to. The rooms beyond the one I was in, together with the scene out of doors, could be represented in harmonious relation with what I was actually looking at. Such representations, however, were more or less a matter of voluntary effort; the spontaneous pictures were usually on the pre-experimental basis. But I was now able for the first time to produce even voluntarily a vivid representation of those parts of my body which could not be brought to view, in proper relation to my sight-perceptions. This was much easier when my legs and arms were in sight, but even otherwise the new representation could still be made. The representation in the old way, though, was the spontaneous one, and doubtless was always at least in the background. But in this older representation there was an unusual paling and weakening of the image of those parts which had most often been seen during the course of the experiment. By bringing my legs and arms into view, the older representation became a sort of torso, the filling in of the seen parts refusing to appear, except in the vaguest way, even by an effort of will. When objects other than the body were in sight, they were not accompanied by any background representation of them on the older basis, unless they gave some sound. In such a case, the sound was localized according to pre-experimental relations, and its source was dimly pictured in accord with this localization.

✓ That the new experience was getting a more stable place in my mind, was perhaps shown by the involuntary recurrence of scenes in their new visual relations, after actual perception had ceased—when I closed my eyes, for instance, or in the evening when my glasses were removed and my eyes were blindfolded.

Fourth Day.—By the fourth day the new experience had become even less trying. There was no sign of bodily discomfort, and for the first time during the experiment, when nine o'clock in the evening came I preferred to keep the glasses on,

rather than sit blindfolded—which had hitherto been chosen as a welcome relief.

During the day, actions appropriate to the new visual perceptions frequently occurred without any conflict or apparent tendency to react by a misinterpretation of visual positions. My hands, in washing, often moved to the soap or to the proper position in the basin, without premeditation or any need of correcting the movement. At one time in the morning, before the bandage was removed from my eyes, I pictured the basin and its appurtenances before me in pre-experimental terms. But my actions were the opposite to those which would have been appropriate to this image. Here I reacted in the new way on an old system of relations, instead of reacting in the old way on a new system of relations—a mode of reaction frequent in the earlier part of the experiment, and by no means fully suppressed even yet. But the more common form of inappropriate reaction now was a movement of one hand when the circumstances really required a movement of the other; as when I reached with my right hand to pick up a book on the floor to my left. I happened to discover, however, a simple means of obtaining without calculation the use of the proper hand in picking up things on the floor—a means which I used thereafter with almost invariable success. If, with one of my feet near the object, I gave a tap or two on the floor before I stooped to pick it up, the proper hand immediately came into play. Curiously enough, it was easier at this time to start the proper foot than to start the proper hand. But there had also been great progress in the suitable use of my hands, shown particularly in the lessened difficulty in serving myself at table, although this was still far from easy.

The sight of objects other than my body, was not accompanied by a representation in the form of the normal experience. The character of the representation of things not actually in sight was influenced by the recency of their visual perception and by the closeness of their relation to things in sight. Objects in sight called up the ideas of neighboring objects in harmonious spatial relation with the things I saw. When I looked down the room in which I was sitting, the ideas of the other rooms of the house were apt to arise in appropriate rela-

tion to my sight perceptions. But if I tried to represent the other rooms without first surveying the room before me and obtaining afresh a powerful 'apperceptive mass,' the spontaneous image of the other rooms was more frequently in terms of pre-experimental vision. And yet the spontaneous representation of things when all sight-perceptions were shut out by closing or blindfolding my eyes, or by darkness, was far from being an inevitable return to the older form of vision. More than once on shutting my eyes, for instance, the room was involuntarily represented as it had just been seen; or in walking after dark into an unlighted room, its general arrangement and more prominent objects rose of themselves before me in the later form of sight. And even in the morning, before I had put on the lenses and refreshed the new experience, the flow of ideas was not purely in the form of the older experience but was strongly mixed with forms of the new. This was also the case on removing the lenses in the evening.

The mode of representing the parts of my body differed with circumstances. On entering the unlighted rooms spoken of above, the movements of my legs and arms were, without my willing it, imaged in terms of the newer sight. As far as I could make out, this quite obscured the older form. At other times, the older representation of my legs striking against the floor was apparent, but seemed dim and unreal as compared with the new. Thus not only was the spontaneous visualization of these parts becoming a mirror of the new visual experience, but the spatial reference of the touch-perceptions was following with greater vividness the direction given by the new visualization. The feeling of contact of things on one side of my body was likewise becoming more spontaneously referred to the proper place in the new visual representation. Hitherto the proper lateral reference had probably always been an afterthought, or reflective reconstruction; the wrong localization was first suggested and then rejected. Now the wrong localization, it is true, still came, but often no sooner than the correct one, and in subordination to this. At other times the older reference alone was suggested. For instance, it occurred that two objects of different shape, one in each hand, when brought into

view, had just the transposed position, as regards right and left, from what I had expected to see them have in the visual field. The touch sensations were here localized in incorrect visual terms.

Sounds coming from objects out of sight were localized as of old, except when the object was vividly represented in the new way. In the latter case, the old localization of the sound was not the exclusive one, but was accompanied by a distinct solicitation to refer the sound to the place where the object was visualized. When the object was in plain sight, the sound seemed to come from the direction in which the object was seen.

The feeling of the inversion or uprightness of things was found to vary considerably with the strength and character of the representation of my body. When I looked at my legs and arms, or even when I reinforced by effort of attention their new visual representation, then what I saw seemed rather upright than inverted. But, if I looked away from my body and gave exclusive force to its pre-experimental image, then everything in sight seemed upside down. Especially was it noticeable that during active movements of the body, as in brisk walking or in coping with objects whose arrangement was relatively unfamiliar, the feeling of the uprightness of the scene was much more vivid than when the body was quiet. During such active operations there was at times a surprising absence of incongruity in the appearance of things. In the evening, during my outdoor walk, I called up a picture of my body in its old visual position, outside the field of view; I had the distinct feeling that such a position was upside down. The outer scene and the new arrangement were clearly at this time the standard.

The swinging of the scene during movements of my body seemed greater or less, according to the way in which I represented to myself this movement of my body. When I pictured the movement in terms of the new visual experience, the movement seemed to be a survey of stable objects. But when I lapsed into the older way of visualizing the movement, then the scene itself seemed to shift before my eyes.

Fifth Day.—At the thought of putting on the lenses, in the

morning, there was an influx of ideas in the new visual form. I even noticed in many cases that there was a reconstruction, in the new terms, of objects which I had just before been thinking of in the old way.

At breakfast, with the lenses on, the inappropriate hand was rarely used to pick up something to one side. The movement itself also was easier and less wayward; seldom was it in an entirely wrong direction. When hand and object were both in sight I did not, as a rule, have to calculate or try to find the direction and extent of movement necessary to reach the object, but merely fixed my attention on the thing, and the hand was laid upon it without more ado, except for an occasional slight correction of the direction.

In walking I did not so often run into obstacles in the very effort to avoid them. I usually took the right direction without reflecting and without the need any longer of constantly watching my feet. When the doors were open I could walk through the entire house by visual guidance alone, without holding out my hands in front of me to warn in case of a misinterpretation of the sight-perception. For the first time, I dared to turn and sit down on a chair without beforehand assuring myself with my hands that I had placed myself aright. My movements were of course still cautious and awkward. And often the question of right and left was troublesome; for example, I wished to grasp the handle of the door beside me, and must hesitate a moment before it was clear which hand to use. But I found that the appropriate hand often came to the appropriate side of the visual field directly and without the thought (frequently necessary before) that *that* visual side meant the *other* side in motor or older visual terms. An evidence of the growing ease with which simple movements were coming to be done is given by the fact that I took a sheet of my notes and laid it upon a shelf in another part of the room, all the while intent on something entirely foreign to the matter in hand.

When I rocked myself in a chair the downward and forward movement of my body was primarily and spontaneously felt as a movement toward the actual visual floor; that is, toward the *upper* region of the visual field, to express the direction in terms

of normal vision. And the backward, upward movement was likewise felt entirely in accordance with the actual visual experience. In this way the rhythmic variation of the visual field during the rocking seemed a harmonious and natural result of the rocking itself, and not, as formerly, a shifting of the scene, unnatural, and therefore suggestive of illusion. And on other occasions, there often was no immediate feeling that the position of the object seen—the position of a person, for instance, with whom I was talking—was incongruous; only after reflection was I aware that the scene was reversed from what it had been before the experiment began.

But in general the most harmonious experiences were obtained during active operations on the scene before me. In rapid, complicated, yet practiced movements, the harmony of the localization by sight and that by touch or motor perception—the actual identity of the positions reported in these various ways—came out with much greater force than when I sat down and passively observed the scene. During such a passive observation I still involuntarily represented my head, shoulders, and chest in the old pre-experimental relation to the actual things in sight. I could, however, by an effort of will fill out the entire form of my body upon the foundation of the parts then seen, but such a visualization was felt to be forced; the spontaneous image of the unseen parts of my body as I sat quiet was thus what it had been during the older experience, and did not at all fit the actual localization of the parts I saw. For these latter were felt to be where they appeared in sight. But even they, when no longer actually in view, often lapsed into the older mode of representation; so that with my two feet pointing in the same direction, but with one in sight and the other outside the visual field, they sometimes felt as though pointing in diametrically opposite directions; the seen foot pointing forward while the unseen one pointed backward, to express the directions in terms of the new visual experience. If I took a fresh look at the hidden foot, however, and then let it pass out of sight, its image remained for some time in accord with the recent perception. But that the older way of representing my body was losing ground, even in the case of the unseen parts,

*Exp. in things
that the visual
movement*

was evidenced by the disappearance of that anticipatory "drawing in" of chin and chest when a solid object passed through the visual field in the direction which in normal vision would have meant a blow in the chest, but which now suggested a free passage overhead. The clear knowledge that the object would not strike me, had been of no avail on former days to prevent some sign of practical distrust.

Localization in cases of unseen contact often went astray, mainly in that the wrong visual side was first suggested, but corrected before I turned my eyes on the thing touching me. Localization of sounds was various, and at times gave a sudden and surprising turn to the experience. Thus, as I sat in the garden, a friend who was talking with me began to throw some pebbles into the distance to one side of my actual view. The sound of the stones striking the ground came, oddly enough, from the opposite direction from that in which I had seen them pass out of my sight, and from which I involuntarily expected to catch the sound. I unhesitatingly accepted the visual directions of throwing and of the stones' movement, but the auditory spatial suggestion was in complete discord with these.

During the usual moonlight walk it was evident that differences of light and shade could not so readily as in normal vision be translated into differences of elevation of the ground.

When blindfolded, after the glasses had been taken off, representations in the form of the new vision were a more vivid constituent of my train of ideas than on any previous night. After I went to bed, while still awake, they came in concrete and colored scenes.

Sixth Day.—In walking about the room blindfolded for a few moments in the morning, images in form of the pre-experimental vision were almost exclusively present. Once or twice at this time a strange indecision and confusion came over me when I did not immediately lay hands on an object which I knew was within reach. I doubted whether I was not using the opposite hand from the one intended. A moment's hesitation, the bewilderment for some reason gave way to assurance, and the movement went on its way. In putting on my shoes—the lenses were now in place—the problem of right and left, which

had hitherto rendered this operation difficult, was unreflectingly solved by making a direct visual comparison of the contours of foot and shoe and seeing whether they matched.

Movements of the head or of the body, which shifted the field of view, seemed now to be in entire keeping with the visual changes thus produced; the motion seemed to be toward that side on which objects entered the visual field, and not toward the opposite side, as the pre-experimental representation of the movement would have required. And when, with closed eyes, I rocked in my chair, the merely represented changes in the visual field persisted with the same rhythmic variation of direction which they would have shown had I opened my eyes. I tried to *make* the imagined objects take the opposite course—the course they would have taken in the older vision during such movements of the body; but only after some moments of effort could I get even a faint suggestion of such changes, and these were immediately supplanted by those in accord with the new visual experience, the instant I ceased my attempt to reinstate the old by force.

When I sat passive, either the old or the new position of my unseen body could be brought prominently forward by act of will. When the old representation was thus reinforced, the actual scene seemed inverted. But when the new representation of my body was emphasized, then the scene felt right side up. During active operations on the visual surroundings, however, the older image of my body became, in many cases without my willing it, weaker than the new, and at times faded completely away.

Variations of touch-localization under different conditions of sight were clearly observable. I felt that my legs were where I saw them, or where they were vividly represented, if they were out of sight. If I tapped upon my knee in plain sight, the contact was localized only where sight reported it to be. But if I tapped while not looking at my knee, the contact was referred to both the old and the new visual positions, the reference according to the older visual experience being probably the stronger. I then placed my two index fingers in view before me, at equal distances from my body, and resting on a paper

tablet in my lap. The right finger now was in that position in the visual field, which in normal sight would have been occupied by the left and *vice versa*; though, of course, the direction in which the fingers pointed in the visual field did not similarly correspond with the old. In many cases, now, a contact (the touch of a pencil point, for instance, by an assistant) on one of the fingers could at will be felt in either of them; at times, indeed, the contact could be referred to both fingers at once. When there actually was a contact with both fingers at once (for instance, a pencil point on one, and the assistant's finger tip on the other), the voluntary transfer of the localization of the pencil's contact from one finger to the other was much easier. And in this case, the contacts, although qualitatively distinguished with ease, and spontaneously referred to their distinct and proper places in the actual field of sight, could nevertheless voluntarily be felt as coming from the same finger at the same time. A movement of one of the fingers, such as a slight bending and straightening of it, while the other remained passive, produced a marked difference between the two fingers, both as to their visual appearance and as to the character of the tactual sensations just mentioned; and this movement rendered the arbitrary reference of the two contacts impossible. Each contact could then be felt only in the place where it was seen to be.

Likewise the substitution of a thumb for one of the fingers (the right thumb for the right index, or the left for the left) prevented a voluntary control of the localization. In the case of the two fingers, however, such a control was still possible when the positions of the fingers in the visual field did not exactly correspond each to that of the opposite finger in pre-experimental sight, or when the contacts fell on relatively different spots on the two fingers, that is, on spots which did not mutually correspond. With the thumb and forefinger, as above described, it is true that, when attention was somewhat withdrawn from vision and given more to touch, I could voluntarily feel my thumb on the opposite visual side from the one on which I saw it; yet there was no reference of the two sensations of contact to the same member, or an identification of the felt thumb with

the seen finger, as was usually possible with the two index fingers. In several cases, though, the visual perception of the source of the peculiar sensation of contact kept also the touch-sensation fixedly on that side where its source was seen to be; or even gave a sudden and surprising reversal to the whole localization, when this had been based on only a vague and partial report from sight. This reversal of localization occurred several times when I was not directly experimenting on the matter, and furnishes an interesting parallel to the results more deliberately obtained. More than once, as I sat with both hands in sight, holding a tablet of writing-paper, a sensation coming from one hand—the feeling of a single loose sheet projecting beyond the others—was involuntarily referred to the visual perception of the *other* hand. But as soon as I saw where the cause of the sensation visually lay, then the touch sensations immediately went over to this latter position, changed hands, in other words, and could not even by effort of will be felt as at first.

Localization of sounds, when the source of the sound was in sight, followed in most cases the visual position of the source, provided I did not voluntarily recall the older position of the object. And since the compass of the visual field was about 45° , the actual divergence from the older localization of the sound could thus be about as great as the diameter of the field of view. For when the source of sound was seen at the border of this field, its older localization would have been on the opposite side of the field and at an equal distance from the center. When the source of the sound was out of sight, a much greater divergence of localization was possible. For in walking I actually felt my feet striking against the floor which I saw extending into the (old) upper side of the field of view before me; and the sound of my steps seemed to come from the place where I felt my feet strike—in this case a divergence of 180° from the old direction of the sound. But when I felt my feet in the old place, the sound too seemed to come from that direction.

In the evening, after I was blindfolded, the play of imagination was almost exclusively in terms of pre-experimental vision.

(To be concluded.)

THE PSYCHOLOGY OF SUFFICIENT REASON.

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§ 1. Among those who make earnest with the idea of genetic psychology it seems to be taken for granted that in some sense the relation between utility and knowledge is a close one—that the extension of the doctrine of Selection into the sphere of knowledge processes, whether as natural selection or selection of a peculiar sort, is warranted. That there is wide difference of opinion, however, as to the nature of that selection and of the accommodations that result, a moment's glance at the literature will show. The uncertainty and differences in the answer to this problem arise mostly from the natural difficulty of keeping the philosophical and psychological sides distinct, in which direction Spencer set an unfortunate example. It seems to be equally unwarranted, however, to consider the question definitely settled either positively or negatively by a one-sided consideration from the point of view either of psychology or of a theory of knowledge. The following paper has therefore nothing more in mind than a consideration of some psychological phenomena which point to a process of selection according to the principle of utility in the sphere of the higher knowledge processes.

§ 2. Genetic psychologists prefer to designate the adaptation of consciousness to its environment by means of intelligence as 'sufficient,' rather than 'necessary,' as in the case of lower psychic organisms. By that distinction they mean to indicate the element of 'subjectivity' which distinguishes the selection in the case of higher will acts from the outer necessity which controls the lower instinctive reactions. Thus Spencer makes a distinction between the 'necessity' of the organized reactions of instinct and the 'sufficiency' of the less stable rational reactions

growing out of the correspondence of ideas to external reality.¹ So also Professor Baldwin: "The principle of sufficient reason is subject to a corresponding genetic expression on the side of accommodation. Sufficient reason in the child's mind is an attitude, a belief, anything in its experience which tends to modify the course of its habitual reactions in a way that it must accept, endorse, believe. This has its sufficient reason, and he must accommodate to it."² With a consideration of the nature of subjective sufficiency is included, therefore, the elements for the solution of the problem of accommodation among intellectual processes.

§ 3. Sufficiency in the sphere of intelligent processes does indeed include much more complex elements than the simple necessity of reflex movement. If the hypothesis of a positive selective factor, over and above the negative function of natural selection, is necessary, as it seems to be, even for the explanation of accommodation in the sphere of reflexes, still more is this positive factor, in much more developed form, a primary requirement in the higher spheres. For, though both are alike in that they are reactions upon environment, they differ materially in the nature of that reaction.

In reflex movement there are two terms, the stimulus and the reaction, between which at least the scientific criterion of *likeness* of cause and effect may be found. They are both objective terms and experience tends to prove the constancy of the relation of stimulus to reaction on the pleasure-pain hypothesis. The higher apperceptive functions, on the contrary, have three terms, the stimulus, the supervening ideal and emotional complex which gathers about the stimulus, and the motor reaction which follows in the will act. Here an entirely new relation meets the eye. Instead of the relative constancy of the relation between stimulus and reaction, instead of the relative constancy of outer conditions, appears a practically absolute inconstancy. The number of possible complexes of ideas and emotions that gather about the stimulus is, to all intents and purposes, infinite. For the stimulus does not work directly as outer reality; but in

¹ Spencer, '*Principles of Psychology*,' Vol. I., Chap. 7.

² Baldwin, '*Mental Development*,' p. 323.

its place enters the complex 'motive,' which, though it stands in the place of outer reality, does not necessarily correspond to it, but oftener does not. The pleasure-pain hypothesis is not directly applicable, for the reason that pleasure and pain do not enter necessarily into these complexes, but are oftener merely suggested.

§ 4. We may, therefore, express the relation (*a*) between the 'motives' and the will act, or (*b*) between the subjective ground of a judgment and the judgment itself as sufficient reason, but not as necessary cause as in the relation of stimulus to reflex movement. This infinite variability of motives which allows us to speak of them as subjectively 'sufficient' but not as causally necessary is evident if we consider with what difficulty 'motives' objectively necessary are found for the simplest will acts.

The consequence of this uncertainty is that we confine ourselves to simple primal effects such as love, hate, etc., which we have, in a manner, objectified as real forces, or at best we make hypotheses on the analogy of our own experience. The personal equation of sufficiency is further observable in spheres not directly connected with the will—in the æsthetic and intellectual judgment. In all thought products the sufficiency lies not in the logical texture, but in the ethical and æsthetic feeling sources of the production. Almost every bit of original thought, especially where it is of the genius rank, must suffer the elimination by critical thought of just those subjective elements in which for the thinkers the sufficiency lay. The same is true in the reaction of the individual upon race beliefs and customs, speech, etc.; the personal equation is always the source of the sufficiency which determines his reaction. 'Characterologie' is, however, notably the despair of empirical science simply because of this law of infinite variability. To be sure, it has been sought to construct a psychology of metaphysical systems, but scarcely with success, even in the case of the non-school philosophers who carry their hearts on their sleeves. The important point is that if the law of selective accommodation is carried up into the sphere of intellectual functions, as a principle of explanation for the existence of our knowledge, the problem becomes extremely complex, because (*a*), as has been shown, the

reaction is no longer upon simple reality, but upon an intervening *motive* complex which shows infinite variations from reality, and (b) as a consequence of this infinite variability, instead of the law of simple 'autogeneity' of ends in instinctive reactions, we have the law of heterogeneity of ends as the governing principle of the higher psychological processes.

§ 5. If it were asked what in the nature of our psychological organism gives rise to this divergence of the motive, which takes the place of the stimulus, from the known reality from which the simple stimulus arises, the answer would come from almost every reader, the presence of the imaginative processes. To these is due the presence of such a law as that of the infinite heterogeneity of ends. If the simple stimulus, unmodified by imagination, was reacted upon, the conditions could be comparatively constant as in instinctive reactions. By imagination is meant, of course, not the vulgar conception of the phantasy which confines it to the sphere of the æsthetical shine nor of the narrow view of some psychologists which restricts it to a particular kind of apperceptive processes, but rather is it a term for that general *element in all apperceptive processes of a complete nature which selectively projects ideas before consciousness* in an emotional unity and sufficiency more complete than that of the merely associational relations. This conception is in full accord with the doctrine of Wundt which describes all those unitary complexes of ideas and feelings (*Gesamtvorstellungen*) which precede either judgments or will acts as the products of 'Phantasie-Thätigkeit' and its 'schöpferische Synthese' which he will have recognized as a thoroughgoing principle of all psychological processes.¹ That this general element of imagination is the source of the divergence of the motives as ideal content from reality is clear from the nature of these processes, by means of which our stimulus may bring about an infinite variety of imaginative complexes dependent upon the nature of the psychological organism.

§ 6. But it is exactly this characteristic of the imaginative processes which suggests them as a possible basis for a doctrine of accommodation. It is true that in imagination we see the

¹ *Grundriss der Psychologie* (1895) p. 367.

source of the divergence of motives from the real environment for which they stand; but in this very divergence is likewise seen the possibility of new adaptation, for this law of the heterogeneity of ends which has its root in imagination offers at least the material for new selection, if only there exists a principle of selection adequate to the demands made upon it. For this principle we need not look beyond the imaginative processes themselves; in their activity lies also a principle of selection which counteracts that element in imagination which works as a source of estrangement from the outer environment, or, if the expression be allowed, uses it as an element in a higher synthesis. The imaginative processes stand in marked contrast to the associations from which they rise in two particularly noticeable characteristics.

a. While the associations pass in succession, according to immanent causal laws, the imaginative processes are governed by a law relatively superior to the associational flow of ideas, by an immanent teleological principle, which, although it expresses itself in the already mentioned law of heterogeneity of ends, yet is at bottom ruled by one motive, namely, the reproduction of reality or the production of experiences analogous to reality. This '*Imaginatio*' is a struggle to reproduce reality by an imitation on the basis of the scattered feeling, and idea, memories which already exist in consciousness. The result of this is a feeling and ideal complex which possesses as its ground tone a 'reality feeling' very like to that of an actual experience.

b. As a consequence of its being governed by this motive, the process of imagination is marked by a certain wilfulness with which some associations are selected and others rejected, according to the criteria of this reality feeling. With this wilfulness comes a certain increase of motor energy, an excess which tends to express itself in actual will acts.

§ 7. A little reflection will suffice to show that these imaginative processes, thus described, are splendid attempts at association in a complete sphere of manifold association. These associations in their mechanical state, if not organized in the form of instinct, stand rather as a barrier to direct reflex accommodation to environment. They must first be brought into a

unitary complex of feelings and ideas, which shall at least relatively reflect the reality which comes to consciousness in the form of stimulus. The ruling criterion is the feeling of reality with which the imaginative complex, this imitation of reality, is clothed. This sense of reality, or 'sufficiency,' it is evident, belongs alone to the feeling side of the complex, for the necessary relations of the ideas come to light first through reflection upon the results of the process, either in the judgment or in the will act, and its relation of advantage and disadvantage in the environment. Until the judgment or will act actually takes place and is reflected upon as a part of objective knowledge or of actual objective reality, that is *retrospectively*, it appeals to consciousness only as subjectively sufficient. For the sense of reality which attaches to the imaginative processes, as background to the judgment or act, arises from the fact that there has been reproduced in consciousness the same organic state (or at least with only slight modification) as existed at an earlier time when reality was directly reacted upon. This means, of course, that the same general affect tone, together with the particular feelings of that experience, have been reproduced by a new stimulus, and consequently that stimulus, by reason of the emotional complex gathered about it, is sufficient to bring about the habitual reaction or one nearly like it.

From these considerations arises a distinction which is fundamental to the whole problem of genetic psychology, namely, the difference between the motor side, which has its source in the feelings, and the immanent relations among the ideas; a distinction which is to be made in every psychological process, especially in the imaginative processes. Both the idea and the motor expression are parallel results of the one psychological process, but stand in no relation of cause and effect. The ideas are not motives to the will act, much less are they causes of the affect side of the process, but both are results of a common, more primal process of imagination.

§ 8. With this distinction, between the 'affect' or force side of the process and the ideal complex, we have a principle by means of which we may more clearly understand the motor expressions which result upon the imaginative processes. When

once the imaginative intuition of reality, with its affect of 'sufficiency' and reality, has come into existence under the influence of the motive of accommodation to the stimulus, the 'motor excess' of this process may express itself in either of two ways. Either the stimulus upon which the imagination followed appeals so directly to the pleasure and pain feelings, or the reality feeling is of such intensity that a will act follows as its expression, or else these conditions do not exist and the motor excess is turned upon the ideal content in a series of apperceptive analytical processes which determine the relations of the ideas among themselves. In the first case the 'force' of the process has found vent in a will act which brings the organism into direct relation to outer reality, in the form of accommodation; in the latter this natural expression has been retarded or prevented, and the energy is expended upon an analysis of the ideal complex, where the theoretical relation of the ideas to each other becomes the problem. The important point is that both of these widely different results spring out of the common primal term—the Imaginative Processes. Out of the union of ideas and emotional elements which takes place under the motive of the imitation of reality, the 'sufficiency' of both the will act and the judgment arises. The 'sufficiency' lies, in both cases, in the affect side of the complex; the coming into prominence of either the motor expression in the will act, or of the theoretical judgment upon the relations of the ideas, is dependent upon laws which we have now to consider. For just here lies the problem of Selection; if like imaginative processes which work under the teleological norm of an imitation of reality at one time pass over into motor accommodation to environment and again fall back upon their own ideal content, on what principle is the selection made as to which complex shall result in will act and which shall not?

§ 9. Here, it would seem, is the place to call in the simple principle of utility, and properly understood, it seems to us to be the solution of the problem. The subjective 'sufficiency' of the motives of will acts and of the 'grounds' of judgments alike was seen to lie in the affective side of the imaginative processes which precede them. The characteristic of this

affect is that it is a strong sense of reality, made up of the memory feelings of prior experiences. All of these complexes have the feeling of reality, closely related to the reality of perception in some degree, but not all have the affect side predominant, in the sense that it appeals directly to the fundamental feelings of pleasure and pain, as a direct stimulus, and therefore not all are brought directly into relations to the principle of utility. In the place of the more definite sense of utility or disadvantage which attaches to the 'motives,' or the imaginative processes which result in motor reactions upon environment, in those complexes which result in judgments upon the ideas, the concept of general worth or value must be substituted. That is, the reality feeling of the imaginative complex is of such a nature that it is handled as of value or worth to consciousness, but not as so intense as to bring forth a will reaction—that is it does not involve a suggestion of immediate pain or pleasure to the organism.

§ 10. The problem of Selective accommodation may then be stated as follows: How is it possible that from motor reactions, which are based entirely upon their utility to the organism—that is, will acts of accommodation to environment—imaginative complexes may arise which have only the predicate 'worth;' that is, which result not in immediate reaction upon environment, but in judgments as to the relations of the ideas? How, in other words, is the abstract concept of *truth* to be connected with the concrete utility of the particular experience.

The answer to this is to be found in the nature of the imitative process of Imagination. The primary type of this process is that in which the affect side prevails and the consequent motor reaction follows. As a matter of fact, all observations tend to show that the less developed the psychological organism the greater the number of completed will acts in proportion to those which are not allowed to follow their course. The more developed the psychological state the greater the degree of selection manifested in the will acts, that is, the less the emotional complexes are allowed to have their natural motor discharge. It follows that we must look upon all imaginative processes as originally ending in will acts; only gradually did

imaginative complexes arise in which the attention was turned upon the ideal complex which gathers about the stimulus, instead of the stimulus itself.

§ II. Definitely formulated then, a theory of selection which adjusted itself to these facts would read somewhat as follows: Reaction of the organism to its environment in the sphere of intelligence does not take place directly upon the stimulus, but through the mediation of ideal complexes which stand for the external reality. These complexes are of the nature of imitations of external reality in that they are the result of imaginative processes which gather together the experience of the past under the teleological criterion of reproduction of the reality feelings of the past. All of the infinite number of complexes thus possible tend to go over into motor expressions in will acts, that is, in accommodation to environment. Some of these are favorable, that is, the imaginative complexes correspond to reality, and some are not favorable, have not corresponded to actual reality. Gradually the number of imaginative complexes which go over into will acts becomes proportionately smaller by means of this selection, and the number of those which are prevented because they have proved themselves not to be in harmony with the external reality, the reaction upon them having failed to be accommodative, becomes proportionately larger.

Thus arises gradually a sphere of imaginative processes which express this motor energy only in appreciative analytical acts upon themselves in the manner previously described. These relations thus developed are of general worth or truth instead of immediate practical advantage or disadvantage.

The nature of the selection becomes clearer from the consideration of certain pathological cases. Hallucination and illusion are conditions where, or account of hyperæsthesia, imaginative processes retain their reality feeling, although repeated motor reactions upon them fail to be accommodative. The immediate reality feeling, growing out of the intensity of the emotion is so strong that the disadvantages (often the *pain*), of reaction upon the external world fail to modify or destroy the imaginative complex. The normal imaginative complex is, however, subject to modification from the feelings which arise

as the result of the reaction. And herein lies the possibility of new accommodations.

§ 12. But how, it will be asked, can such a theory of selection account for the logical and *a priori* relations among the ideas which tend more and more to segregate themselves from the direct accommodations. Surely they are not the products of selective accommodation and yet an extension of the principle of selection to the sphere of the intellectual processes, must be on the basis of the principle: that only *those ideas are true which have proven to be of utility*. A little reflection will suffice to discover a fallacy in this principle. Ideas are never of utility; only feelings and states which are consequent upon accommodations are of utility. Ideas are only signs for psychological states. To speak of ideas as being of utility implies a point of view which overrides the boundaries of psychology, and falls into the fatal error of Spencer, of basing the whole of genetic psychology on the metaphysical hypothesis of a correspondence between the ideas and reality. This distinction between the 'force' side and the 'ideal' side of the imitative processes, which is expressed in the sentence "the idea does not work but only the process of getting the idea," enables us to separate completely the dynamical and utility side of psychological processes from the logical relations of the ideal content that results. And this is an absolutely necessary presupposition of any genetic study. The fundamental laws of the ideal side of our conscious complexes are laws of relations based upon the analytical criteria of 'clearness and distinctness.' They belong distinctly to the peculiar sphere of ideas and have nothing to do with the problem of organic accommodation. In the latter sphere the criteria, as we have already seen, are distinctly affective, growing out of the feeling of reality and the pleasure and pain which accompany it. The ideal relations as such lie, accordingly, entirely outside the line of direct accommodations. They work only indirectly in future accommodations, in that when consciousness is gathered together again in a new imaginative complex for a new motor reaction, the ideal content appears in more distinct and perhaps modified relations, but again the 'sufficiency' and the accommodation will lie in the affect side.

§ 13. But is not the fallacy in the preceding expression that "only those *ideas* are true which have proven themselves to be of utility" the stumbling block to any application of genetic selection in the intellectual sphere; a final barrier to any connection between utility-selection and truth? Were it not better to say: *our ideas must be true, that is correspond to outer reality, if the acts based upon them are to be advantageous?* Here the correspondence between our ideas and outer reality is assumed and the utility of our acts concluded from the assumption. The primacy of immanent *a priori* relations among ideas is taken for granted as the source of a necessary accommodation to an environment corresponding to these ideas. On the contrary, it could be claimed that ideas must prove themselves useful, before they can obtain a permanent place in the content of our consciousness, they must be seen by actual practice to correspond to reality before they can be distinguished as permanent truth from the mere fictions of the imagination. This apparent antinomy which so often stands in the way of reconciliation of empirical and *a priori* theories of knowledge rests upon different ways of looking at a single process or fact. In the first part of the antinomy is expressed an objective attitude toward accommodations after they have actually taken place. We conclude from a favorable accommodation on the part of a particular psychological organism as a consequence, to a knowledge of the true relations of things in this consciousness as ground. On the other hand, if we say that the ideas must be of utility to be true, we conclude from the subjective ground to an objective consequence, because from our standpoint, as practical agents, it is alone those ideas which appeal to us as of worth which correspond to this practical accommodation which we have made in will act.

§ 14. This difference in attitude corresponds to a distinction which can be made in the general body of truth. The relations among individual elements of scientific truth are true in a sense that the whole of truth is not, for they are analytically determinable according to the logical criteria immanent in the ideas themselves. The whole truth, however, has no such criteria as Descartes clearly saw when he made the whole of the truth de-

pendent upon the certainty of the intuition of the self, that is upon a psychological term of belief. The self cannot be doubted because there are no higher criteria according to which it can be proved. The reality feeling of the self is, therefore, the criterion of the truth of all the content in the consciousness of the self. So also in this case the relation may be expressed epigrammatically in the sentence: *The whole of truth rests upon utility which goes back to the psychological affective side, its parts, however, upon analytical and logical necessity.* This contradiction finds its psychological solution, and that is all that concerns us, in the reduction of both terms to a more primal term, the imaginative processes. These are found to be the background of will acts and judgments alike. The 'sufficiency' of the 'motive' as well as that of the psychological ground of a judgment lies in each case in the affective side of the imaginative complex. Of these two possible results of the imaginative processes, the will expression is the more primal. The relation of the practical will side of consciousness to reality is closer and more fundamental than that of the ideal. In its accommodation, therefore, is to be found the source of all new content in consciousness. The reflective processes which are the result of the turning of the motor force or attention upon the ideal content are the secondary results when the natural reaction is hindered or retarded. Thus arises gradually a sphere of segregated truth, which is first of all of theoretical and general worth, and only indirectly of practical utility. The individual acts of will which are based upon the utility to the organism whose reactions upon environment they are, must tend in the long run to fix the results as necessary for the race. When, however, these results are so recognized, they become parts of a settled and independent body of truth, which has its own laws outside the sphere of the utility reactions which first brought it into being.

§ 15. A study of the development of child consciousness and of primitive peoples would present a mass of material which tends to prove that intelligent accommodation to environment, proceeds upon the principle of a selective *reduction* of imaginative reactions upon given kinds of environment to permanent

bounds. That is in the proportion that *extension* of the possibilities open for the imagination is reduced, in equal proportion, is the *intension* increased. In the young child or in the primitive man the imagination clothes elements of environment of the most divergent nature with the same attributes, mostly personal, and reacts upon them accordingly, or again the same stimulus is at different times reacted upon with different imagination content, simply because the reality feeling does not work definitely and certain. Thus arise the phenomena of superstition—the freedom from which is a continuous process of accommodation to environment, and which, when completed, may leave behind a new science as illustrated in the development of chemistry from alchemy. When such a stage is reached where a definite amount of theoretical material is segregated by the selective reduction of the number of the possible reactions or imaginations, the imaginative processes, though restricted in extension to this material, grow in intension, and the process is then continued in the form of scientific hypothesis. But all this leads us into the sphere of comparative psychology, while our only object was an analysis of the psychological processes which point to a doctrine of selective accommodation.

In closing, the interesting fact may be noted that both Kant and Herbart find the subjective sufficiency of judgments to lie in the imaginative processes. Kant, in his subjective deduction of the categories finds in the transcendental synthesis of imagination the ground of the union of the sense intuition and the logical forms. Herbart likewise finds the psychological grounds of sufficient reason in the imagination. With both, however, the imagination is at bottom a metaphysical term, and, consequently, though both gave valuable suggestions as to the nature of the psychological grounds of judgments, it is only suggestively that their doctrines of imagination can be referred to in this connection. The above developed principle of selective accommodation rests alone on the analysis of the psychological processes called imagination.

SOME FACTS OF BINOCULAR VISION.

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Some interesting experiments in binocular vision were reported a few years ago by Professor Hyslop and Professor Venn in *Mind*¹ and in *THE PSYCHOLOGICAL REVIEW*.² The unusual conditions of vision under which these experiments were performed—both observers are able to carry out the adjustments of ocular accommodation and those of ocular convergence independently—seem to have prevented their conclusions from receiving the usual critical treatment which comes from general and extended experimental observation. A little practice has enabled me to follow the experiments of both, and while I am able to corroborate the results in general, important considerations prevent me from adopting the conclusions which Professor Hyslop reaches in his last paper. These conclusions may be summarized in Professor Hyslop's own words as 'looking to a central explanation of both distance and magnitude, independent both of peripheral conditions and motor impulses.' It is the aim of this paper to report certain experiments which seem to point in a different direction, and it will be possible, I think, to show where the error has crept in.

The apparatus for the experiments consists of two plane mirrors mounted in two frames which are hinged together in such a way that the mirrors may be inclined at various angles. Let ad and bc be the mirrors. (Fig. 1.) They may be folded so as to come into the positions $a'd'$, $b'c'$, or so that their positions are $a''d''$, $b''c''$. The whole may be held in the hand at a convenient distance from the observer's eyes. At the beginning of the experiment the mirrors are held in the same plane adb ,

The eyes are converged in the directions me and nf , so as to

¹*Mind*, Vol. XIII., p. 499; Vol. XIV., p. 251 and p. 393.

²*PSYCHOLOGICAL REVIEW*, Vol. I., p. 247 and p. 281.

receive the reflected rays from a luminous point o , the relative positions being so chosen that the pencil of light entering the right eye comes from the right mirror, and that entering the left eye from the left mirror. The points seen will be referred to a distance behind the mirror as great as that of the real point in front of the mirrors. If now the frame be slightly folded so as to bring the mirrors into an inclined position, with the angle of

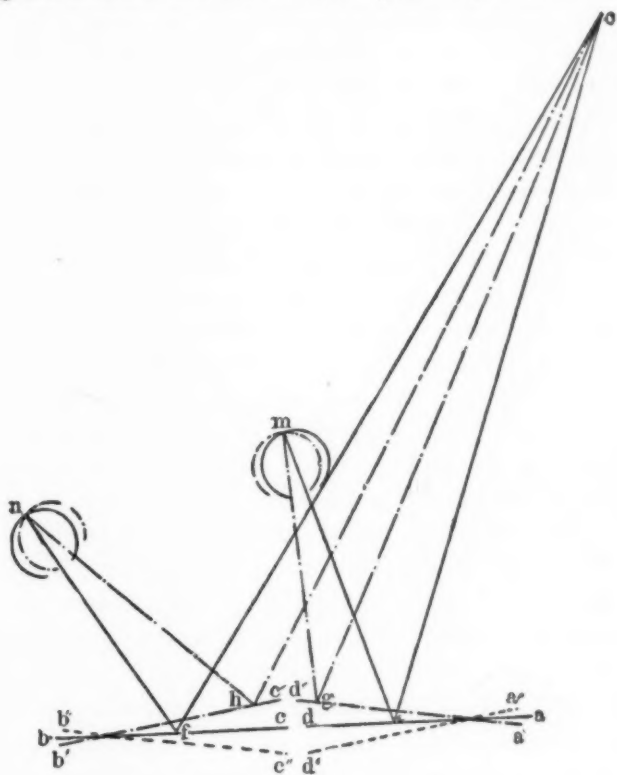


FIG. 1.

inclination turned toward the observer, as $a'd'$ and $b'c'$ ¹, the points of incidence of the rays entering the eyes will travel from e to g and from f to h . The effect of this movement on the apparent distance of the point in depth will be imperceptible, as can be shown by closing one eye while the mirrors are being inclined. When, on the other hand, both eyes are fixed on the point, as the mir-

¹ The angle is exaggerated in the figure.

rors are slightly inclined, the point behind the mirrors is distinctly seen to approach the observer. The eyes must be converged in the directions mg and nh , and the fixation point evidently lies very much nearer the mirrors than when the optical axes were in the positions me and nf . A very slight inclination of the mirror produces a marked effect. Just the opposite phenomena follow an inclination of the mirrors when the angle of inclination is turned away from the observer. Here the point is seen to recede during the movement of the mirrors. The point of intersection of the optical axes also recedes. The whole series of phenomena is evidently explained by the fact that objects requiring a greater degree of convergence are judged to be nearer, and those requiring a smaller degree of convergence are judged to be more distant. It is to be noted that the judgments of position are certain only during the actual movements of the mirrors. As soon as the movement ceases the point seems to have that same sort of indefinite location in depth which is so characteristic of our judgment of the distance of the stars.

New and important experiences appear if an object is used instead of a luminous point. When the mirrors are now inclined into the positions $a'd'$, $b'c'$, the object, as the point before, seems to approach the observer, but it also grows very distinctly smaller. This diminution in the size of the image can evidently not be due to the fact that the points of incidence travel from e to g or from f to h , for if this slight change has any effect at all, and it is so slight that it doubtless has no such effect, it would be in the opposite direction, for since the object is virtually brought nearer by the inclination of the mirrors, its retinal image is thereby increased in size. The decrease in apparent size is connected with the apparent approach. The whole matter will be clear if we recall the ordinary facts of perspective. If two objects unequally distant give the same sized retinal image, the more distant object will be the larger and a long series of experiences has taught us to judge in accordance with this fact. In ordinary experience, then, when an object approaches an observer the convergence will increase and, at the same time, the image on the retina will grow larger. But, under the conditions of the experiment, the retinal image remains constant (or, if

anything, grows only slightly larger), while at the same time the convergence is increased. The lens does not change its degree of accommodation, so that the case is not complicated by any factor besides those of retinal image and convergence. There is evidently only one objective phenomenon which could give this unusual combination of retinal image and convergence, and that would be the approach of an object which was rapidly becoming smaller in size. The result is that we actually perceive an object in the mirrors which approaches and at the same time grows smaller. The converse may be seen by folding the mirrors slightly away from the observer; the object now seems to recede and to grow larger. The explanation is of course similar.

While the convergence is actually changing the appearances of movement in the object are very apparent; as soon as the movement of the eyes ceases the absolute distance of the object in space becomes more indefinite, just as in the case of the point in the first part of the experiment. The diminished size of the object, on the other hand, remains unmodified. This justifies us in concluding that the apparent magnitude of objects is due to the combination of retinal images and sensations of convergence under the general law that *of two objects requiring different degrees of convergence and yielding the same sized retinal images, the one requiring the greater convergence will seem smaller*. It will also appear nearer unless associated factors from past experience come in to disturb the localization. These associated factors are not strong enough to affect the judgment while sensations of movement are actually coming into consciousness, but may have some influence when the only sensations from convergence are the somewhat weaker sensations of position. In any case the effect of the two peripheral conditions, namely, retinal images and motor sensations (including sensations of mere position) are the determining factors. These factors, being combined in unusual relations, give rise to unusual perceptions. But the perceptions are in accordance with the ordinary rules of perspective as shown above.

All the above described facts may be easily observed by any one. The following experiments require in their second modi-

fication some ability to dissociate the closely related processes of convergence and accommodation, but an observer with strong eyes and a little practice can soon acquire the ability to perform them. The same pair of mirrors is employed, but they are so inclined that the angle towards the observer is considerably less than two right angles, as ad , cb (Fig. 2), and the eyes are so located that the only ray from the luminous point o which is visible in the left eye is incident on the right mirror, and

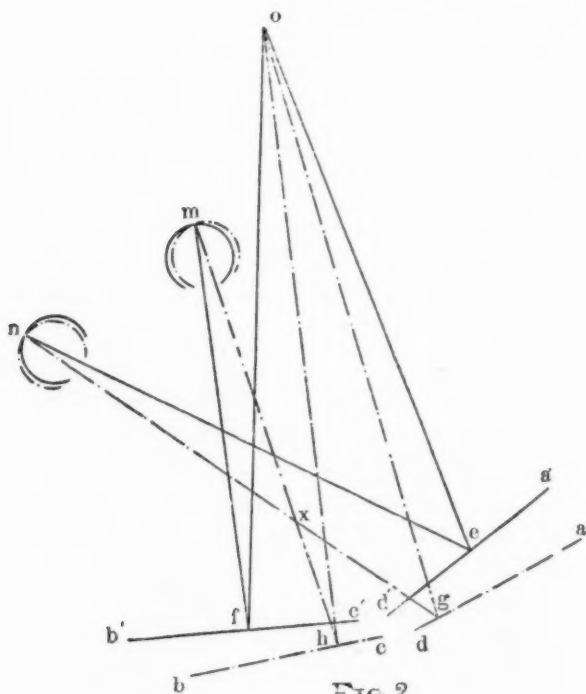


FIG. 2.

the pencil entering the right eye is incident on the left mirror. The only way in which the two images can be made to appear single is by converging the two eyes so that their optical axes shall intersect at the point where the two pencils of light intersect, that is, at x . The two eyes must be converged in the directions mh and ng . If this is done the point will be seen as single, but its location in space will not be at all definite. It seems to be behind a third mirror situated between the two

original mirrors which are visible in indirect vision. If the angle towards the observer be made smaller, as $a'd'$, $b'c'$, the point will be distinctly seen to approach the observer. The points of incidence will travel along the mirrors from g to e and from h to f , but this change can be entirely neglected as in the earlier experiment. The opposite effect will be observed if the angle toward the observer is gradually increased; the point will then recede in a very noticeable degree. A reference to the figure will show how the change in angle of the mirrors is accompanied by a change in the degree of convergence. In all the cases described active movement of convergence is always accompanied by a decided appearance of change in the distance of the object in the third dimension, and this change in apparent distance always follows the rule that the greater the convergence the nearer the object. The rule holds without exception for relative degrees of convergence; when, however, the absolute distance is to be judged, other factors enter in and the object seems further away than the real point of fixation. This false reference of the point of fixation is doubtless due to the conditions which arise from the imperfect reflection of the mirror which gives added sense data, and to the conflicting sensations of accommodation to be discussed more fully in the next modification of the experiment.

As in the first series of experiments, important factors are introduced when we make use of an object rather than of the luminous point. A new complication arises in the fact that when the eyes are converged to the point x they will, under the ordinary circumstances of vision, also be accommodated so as to focus rays of light diverging from x . If an object whose rays of light are less divergent, as in the case of the real reflected object, is to be seen in clear outline the accommodation must be changed so as to adapt it for a point whose distance is greater than the point of fixation. That is, the accommodation must be for distant objects while the convergence is for a near object. This is difficult for the unpracticed observer and may be impossible for some. When the ability to accommodate and converge independently is once acquired, however, the object can be seen very clearly and sharply defined, even while the eyes are con-

verged to the nearer point. If now we start with the mirrors in the position *ad*, *bc*, (Fig. 2) the object will be seen as very much smaller than the image in the plain mirror when observed with the single eye. Its relation to the position before convergence took place will be rather indefinite, but seems at first somewhat greater than before. If the angle is made smaller, as *a'd'*, *b'c'*, the image seems to grow very much smaller and approaches decidedly. As soon as the movement stops the location of the object again becomes indefinite, and it may appear at the same or even at a greater distance than before. The conditions are very much involved and yet the results all obey the principles that during active movement of convergence the greater the degree of convergence the shorter the apparent distance of the object, and, the retinal image remaining the same in size, the smaller the apparent size of the object. Here again, when the movement ceases the diminished size remains constant while the localization becomes less definite. The fact that the distance seems to be about the same when the mirrors are at rest, whatever the size of the object, speaks for the influence of the sensations of accommodations which are of no very great importance in the estimation of depth, but probably play some part. We shall find evidence later for assuming that accommodation has some influence in perception. The more important fact that while the relative position corresponds to the convergence, the absolute localization is not at the point of fixation, furnishes a greater difficulty, but here again it is to be noticed that the mirrors seen in indirect vision are smaller and the illusion of greater distance could easily arise, as it often does, when a concave lens is held before an object; the object is seen smaller and further away until the attention is called to the true relation of the image to the object.

There is only one point in which this series of experiments differs from the first, and that is in the dissociation of convergence and accommodation. The size of the retinal image here remains constant just as in the former series. This follows from the fact that the image is sharply focused on the retina, and since the rays from the object are equally divergent whatever the position of the mirrors, the lens must remain constant if the

rays are always to be brought to a focus. That the size of the aperture in the pupil can have no influence on the size of the image followed from the general law of refraction that a part or the whole of a lens casts exactly the same sized images.

We turn now to the discussion of Professor Hyslop's experiments and conclusions. The earlier series differs from those which have been reported in this paper, in the fact that the figures were there drawn on paper or glass and the possibility of comparing a large number of successive experiences was thus lost. The experiments here described furnish important additions to the general body of fact which may be used in explanation, but even the less elaborate experiments with fixed figures seem to lead to conclusions which are favorable rather than adverse to the motor-sensation theory. In fact similar results have been used by Aubert,¹ Professor Le Conte,² Professor Martius,³ and Dr. Rivers,⁴ to establish the same conclusions that I have drawn from my experiments. Professor Hyslop's experiments are in brief as follows: Two circles are drawn at a distance of a few inches apart, and the eyes are converged so as to fuse the images, either by crossing the eyes or at a point nearer than the plane of the paper, or by distant convergence at a point beyond the plane of the paper, as represented in figures 3 and 4. *A* and *B*, *A'* and *B'* are the circles in profile. In order to get clear images at *C* and *C'*, of course the accommodation must be unnatural. The result of crossing the eyes is that *C* is seen in direct vision considerably smaller than the original circle, and nearer to the observer's eyes. *A* and *B* are seen in indirect vision somewhat larger than *C*, but smaller than the original circle, not so near the observer's eyes. All of these results I am able to corroborate fully. I find also the converse of these facts when the eyes are converged at a point beyond the plane of the paper, as does Professor Hyslop. There is another important observation which has evidently not escaped Professor Hyslop, since it appears in his figures, but which he seems to have made

¹ *Physiol. des Netzhaut*, p. 330.

² *Sight*, p. 158 seq.

³ *Philosophische Studien*, Bd. V., p. 601 seq.

⁴ *Mind*, N. S., vol. V., p. 79.

no use of in his explanations. This is the observation that the distance between A and B , as seen in indirect vision, is very greatly increased; in fact, just about doubled, so that if we

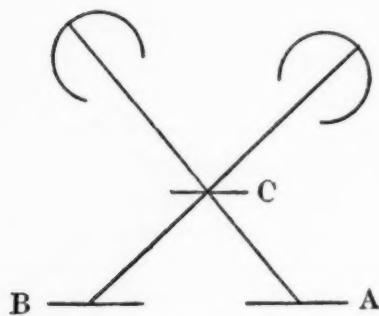


FIG. 3.

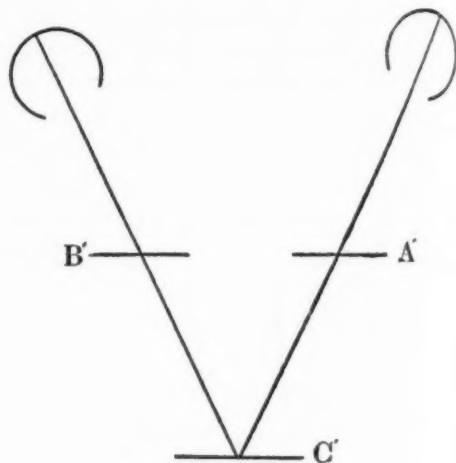


FIG. 4.

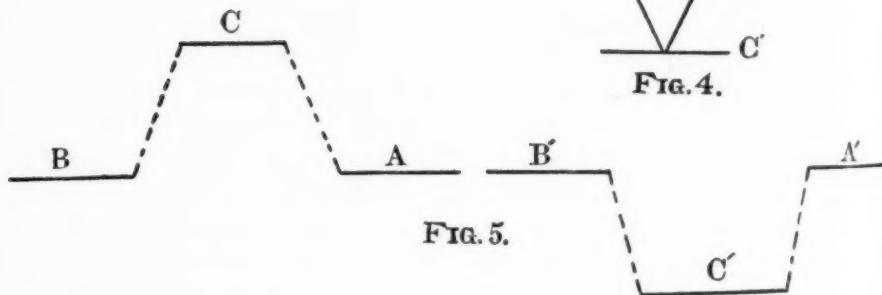


FIG. 5.

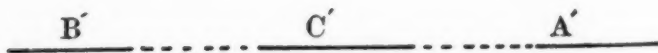
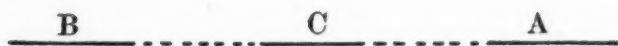


FIG. 6.

think of C as lying between A and B , the distance AC and the distance BC are about equal to the original distance AB . Furthermore, Professor Hyslop, while he has been at great

pains to discuss all of the possibilities of some change occurring in the size of the retinal image, seems to have overlooked the possibility of an explanation without the assumption of any change in the size of the image. But, since the image is sharply defined on the retina, the lens must be accommodated as it would be in monocular vision. There is therefore no ground for the long discussion as to the possible changes in the image due to accommodation. Still less is there reason for refuting the supposition that difference in aperture would affect the size of the image. The oblique distances from the eyes to the circles in both cases (Figs. 3 and 4), are slightly different from the perpendicular distances, but the differences are not appreciable and have no perceptible influence. The smallness of C in the first case and the increased magnitude of C' in the second case offer no difficulties in the light of the explanation given of the results with the mirrors. The retinal image is constant in size, the convergence is different, and the object which is, on account of the convergence, perceived as nearer in the first case is interpreted as smaller, while in the second case it is more distant and interpreted as larger. The estimation of absolute depth is very indefinite, but may be made clearer by bringing up some small object, such as a pencil, in the plane of the paper. The difference in the size of the circles in indirect vision and the central images furnishes a more complex phenomenon. It is necessary to bear in mind that we are dealing here with a case, which is essentially a case of monocular vision. Yet the binocular influences are present and must play some part in determining even this monocular perception. That the binocular and monocular tendencies are in conflict, appears from the fact that the circles are pushed farther away from each other, that is, the distance AB in indirect vision is very much increased. This increased distance will seem to grow shorter if the attention is turned toward one of the circles visible in the indirect field. Professor Hyslop has pointed out that the distance in depth of the central circle and of those in indirect vision is apparently different; the indirectly seen circles appeared to be nearer the plane of the paper. The apparent increase in the distance apart is due to an illusion, as the result of which the

perspective distance is mistaken for the horizontal distance. What is really seen is represented in Fig. 5, what is thought to be seen is represented by Fig. 6. This illusion is due to the indistinctness of indirect vision and tends to disappear when attention brings out the perspective. The differences in size of the indirectly seen circles when compared with the original circles may be explained largely, if not completely, as the influence of the accompanying binocular sensations on the monocular perception. The circles are seen as somewhat nearer and consequently smaller in the first case, as more distant and larger in the second. Apart from the special complication here pointed out, these phenomena are perfectly analogous to those which appeared in the experiments with the mirrors. The explanation may be extended so as to include certain other cases which Professor Hyslop uses in his criticism of the association and motor-sensation theory.

The case of after-images remarked by Professor Hyslop and independently reported by W. Scharwin and A. Novizri in the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, Bd. XI, Hf. 5, furnishes a striking parallel. An after-image appears larger the more distant the plane on which it is projected.¹ The retinal image is in such a case exactly the same size whatever the distance of the plane may be. The change in apparent size is to be explained in the same way as in the cases described.

Other facts, derived from the fusion of stereoscopic figures under various conditions, furnish, in Professor Hyslop's view, insurmountable difficulties for the motor-sensation theory. If two stereoscopic figures made with circles in such a way as to give the frustum of a cone when fused by crossing the eyes, be drawn on separate pieces of paper so that the distance between the figures can be changed by moving the papers further away from each other, or nearer to each other in the same plane, the results will be the following. "Thus we move the circles farther apart while increasing the convergence to retain fusion, the frustum shortens while its magnitude diminishes. On the other hand, as they approach each other and the fusion is sustained,

¹I find the fact mentioned by Aubert as a discovery of Lehot (*Fechner Repertorium*, 1832).

the frustum lengthens and the magnitude increases, and all this while the figures occupy the same plane." The increase in

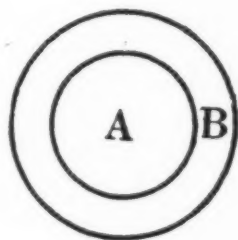
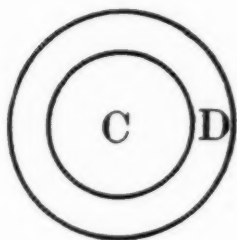


FIG. 7.

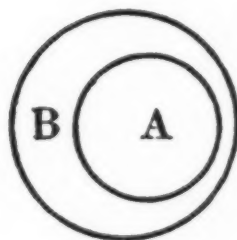
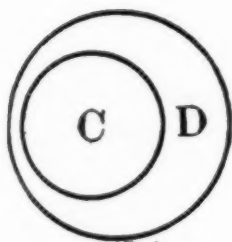


FIG. 8.

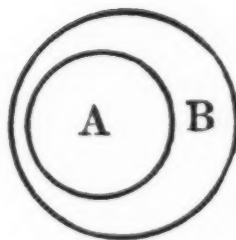
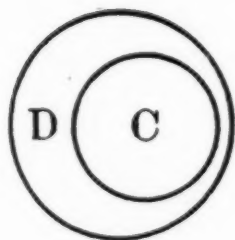


FIG. 9.

magnitude of the circles offers no difficulty in this case. The actual change in the distance of the figures from the eye as they are moved in a given plane may be of some slight influence but

this factor is not appreciable; the retinal images are practically constant in size. The variation of the fixation point, which recedes when the figures approach each other and advances towards the observer when the figures are drawn apart, sufficiently explains the change in the apparent size of the circles. The length of the frustum is another matter. Under ordinary circumstances this decreases as the object recedes, so that when an object recedes to an infinite, or even to a very great distance, all appearance of solidity is lost. In the case in hand, the object in question does really recede when the figures approach each other. The spaces between the circles will share in the enlarging effects of this receding movement, but when the frustum is spoken of as lengthening reference is not made to this increase in length taking place concomitantly with the other dimensional changes. The length of the frustum increases relatively more rapidly than it should to preserve the original proportions. This increase is still more important when we think that under normal conditions the frustum would naturally become proportionally even smaller. The explanation of this change in the length of the frustum is to be sought in the binocular parallax. This can be shown by the familiar fact that four circles drawn as in Fig. 7, where *A* and *B*, and *C* and *D* are concentric, when united by crossing the eyes give no stereoscopic effect whatever; the binocular parallax is practically zero. When the binocular parallax is positive, as in Fig. 8, the result is a frustum of a cone with the small circle towards the observer; when the parallax is negative as in Fig. 9, the result is a frustum of a cone with a large circle nearer the observer. As the positive or negative parallax is increased the frustum grows longer as may be shown by separating the centers of the circles *AB* and *CD* more and more.

The binocular parallax under the conditions of the experiment with which we started, increases when the figures approach the median plane as will be seen by referring to Fig. 10, where the angles *anb*, *bnc* and *cnd*, are larger than the corresponding angles *a'nb'*, *b'nc'* and *c'nd'*; *abcd* represents here the profile of two such circles as are represented in Fig. 8. The first position *abcd* lies nearer to the median plane than the second position *a'b'c'd'*. The point *n* represents the nodal point

in the eye. The size of the retinal image will undergo some changes also when the circles are moved away from the median plane, but these changes are not of importance when the distance through which they are moved is small. *The lengthening of the frustum is therefore a function of the visual angle and increases when the figures approach the median plane.* A similar result appears when the object recedes in depth from the observer, the binocular parallax will decrease as the distance

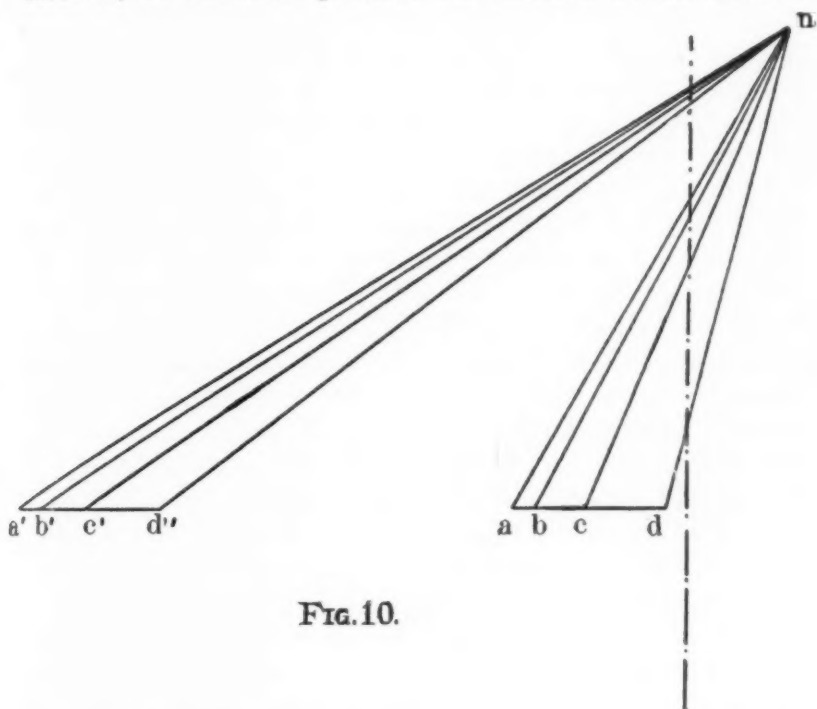


FIG. 10.

from the eye increases. Of two equal variations, however, one in the lateral direction, the other in the third dimension, the former will have the greater influence in modifying the binocular parallax. This proposition can be mathematically demonstrated for all distances great enough to come into consideration for our experiment. These established results explain another series of facts which Professor Hyslop has described. If two stereoscopic figures are drawn at a given fixed distance and moved backward and forward in the third dimension, the rela-

tive size of the circles will remain constant, but the frustum will increase in length as the figures move away, and it will grow shorter as they approach. The fact that the size of the circles seems to remain constant is what we should naturally expect. When fusion once takes place the size of the image is determined by the relation of the size of the retinal image and the degree of convergence. If now the figures are moved away, the convergence and the retinal image vary just as they would if a real object at the point of fixation were being moved away. The relative size, therefore, seems to remain constant. Not so with the binocular parallax. The figures are at a fixed distance apart, and when moved away from the eyes they will approach relatively nearer to the median plane. At an infinite distance they would be on the median line, and near at hand their distance from that line reaches its maximum. This approach to the median plane when the figures move away gives a relatively smaller decrease in the binocular parallax, a result which is in contradiction to all ordinary experiences, for usually when an object moves further away the binocular parallax decreases without this counteracting influence. Here again, we are confronted by a series of conditions which seem contradictory to experience. The interpretation of the sense data will, however, be fully determined by peripheral conditions. The object observed will seem to change, for that is the only possible objective condition under which the unusual combination of sense data could possibly be presented.

All of the results from these various experiments furnish ground for accepting the association and motor-sensation theory of visual space rather than the contrary as Professor Hyslop concludes. The sense-data presented in every case are interpreted in accordance with past experiences. Where such combinations of data arise as are not in conformity with any single past experience, the interpretation immediately permits the assumption of a change in the object itself; the size of the object changes or the position of its parts in the third dimension seems to vary. The relation fixed by past experience between the various sense-data is more constant than the belief in a single particular case, so that, although we know that the object re-

mains constant in size, it is interpreted as changing, this perception being more readily adopted than any modification of the fixed relation between the various kinds of sense-data. The light thrown by this fact on the general theory of space perception as well as on the question of perception in general requires more discussion than can be allowed after the detailing of these empirical facts. In general, however, the conclusion is to be emphasized that analysis of the phenomena furnishes striking evidence in favor of the motor-sensation theory rather than against it.

SHORTER CONTRIBUTIONS.

BLOTS OF INK IN EXPERIMENTAL PSYCHOLOGY.

The chance characters made by the compression of one or more drops of writing fluid between two small squares of paper seem to have a varied usefulness in experimental psychology. Rectangular pieces of paper twice as long as wide are folded transversely in the middle. Six centimetres by three is a convenient size for use when large series are employed. Small drops of rather thick common ink are then placed near the centre of one of the squares, and the two halves firmly pressed together with the moving fingers until the fluid has been absorbed. The shape and size of the blot may be determined to some extent by the finger and by the amount of ink applied; several small drops make more various blots than a large single drop.

There being no proper top or bottom to these characters, mere partial or complete reversal changes their apparent nature. Thus the two originals may be used as reverses of each other directly, or by quarter, half, or three-quarters turning of one of them, three other relative characters may be produced. If circular bits of paper instead of square be employed, theoretically infinite combinations are at the command of the experimenter. Direct reproduction of any character may be made by tracing its outline and filling this in with a brush and pen. If many reproductions are required, photography is the best means, and the negatives used in that process may be useful as stencils, behind which variously colored papers can be placed somewhat as in the ambrotypes of fifty years ago. If the blots be required in series, they are best made on heavy gummed paper squares and stuck upon sheets of the required shape and size. Paper not too smooth is best for the blots, that they may dry quickly and be colored uniformly. If copies larger or smaller than the original are desired they may be made with the pantagraph. Colors, of course, are as easily used as black, and variation in the way of shading is also unlimited.

The characters may be exposed behind a Münsterberg pendulum, attached by rubber bands to a kymograph cylinder, through an aperture, and used in many other ways.

It is suggested that these characters may be of use in at least the

following psychological researches: In the study of the content of consciousness as regards the relative ease of recognizing an object and its reverse, either when seen alone or in various series. In studies of memory, by measurements of the length of time after which a given blot, straight or reversed, may be recognized; also by the relative power of reproducing after an interval the outline of an exposed character. In the study of Imagination, qualitatively, in various ways, and quantitatively, by measurements of the relative times required for a presented suggestive blot to bring to mind its obvious likeness. In determinations of reaction time with choice. In study of the discrimination of minute formal differences. In the study of after-images of various colors, and positive or negative. In studying Association.

The advantages of blots or characters thus made seem to be these; The practical infinity of their variety; the ease, rapidity, and cheapness of their production in black or colors; the facility with which exact reverses are made; the lack of associational suggestiveness of many of them, and on the other hand the ease with which suggestive ones may be obtained; and the unlimited range in size.

HARVARD UNIVERSITY.

GEORGE V. DEARBORN.

THE IMAGERY OF ONE EARLY MADE BLIND.

I became blind at the age of five years and one month, in August of 1877. My home was then in New Brunswick, Canada. I have an image in my mind of many of the scenes which I saw before losing my sight. I remember how the trees looked across the river where they seemed to disappear into the sky and I believed they supported the sky. I remember how the small ships which used to come up the river looked. I also have a vivid picture of the falls in the river. I used to sit on the edge of the bank overlooking the falls and gaze down about fifty feet at the water. In fact I remember almost everything which I saw during the last summer while I possessed my sight.

My Idea of Space.—When I contemplate a geometric proposition, it is presented to my mind as raised on a piece of paper. When I studied geometry I had all the diagrams used to prove the proposition raised on paper. Thick pasteboard was used so that the figure would endure. Any geometric proposition, therefore, appears to me raised on such a figure as I then used.

When I wish to represent to myself something infinitesimally small, I take a thin piece of paper and tear it in halves; then I tear one of the halves in halves and continue this process until I have the smallest

piece of paper which I can hold in my hands; then I consider that subdivided as many times as I subdivided the original piece of paper, and then again what is obtained by that subdivision again subdivided, and so on until I can think no longer of the subdivisions for mere infinity of numbers, and still I do not feel satisfied when geometricians make a leap in the dark from this smallest conceivable to zero. I can not understand how zero can ever be reached in this way. The longer the piece of paper which I at first divided is the more thoroughly can I approach to the minutest possible in the repeated subdivisions.

When I wish to represent to myself the infinitely long I consider myself in an extended body of water with a life preserver on. I have sometimes been in the water in such a manner; and if I can hear no one on the shore, there comes to me some idea of what infinity is. My home is on the shore of Lake Superior and when in that body of water facing away from the land, there comes to me some idea of infinity.

I consider infinity going away just as I would swim away from the land. I might swim and swim and still I would never come to a stopping place in that body of water; and infinity would go on before me to the opposite shores of the lake four hundred miles away and still it would continue beyond that point an infinite number of times. Or again, I represent infinity to myself as the rails of an extended railroad track. When I wish to represent to myself two parallel lines which, however, far produced will never meet, I consider the lines of the track. I have frequently followed these rails for a long distance, fourteen miles being the farthest I have ever gone at once; and from this distance I can consider those rails continuing on in that same line with that same distance between them for an infinitely greater distance than I have ever walked.

Of course I was not old enough when I lost my sight to consider infinity, but I do remember looking up into the sky and wondering what was beyond that and how far it went. I distinctly remember seeing a ball thrown up into the air as far as I could follow it with my eyes, and from that I got my only seeing conception of infinity. I lost my eyes with scarlet fever, and before the sickness came on they were as perfect as any eyes.

I cannot consider in my mind at once a figure of more than six sides. If I wish to consider more sides than that, I have to consider them in parts of three or four sides at a time. I have to go round the figure in my mind slowly. I cannot conceive in my mind at once a polygon of an infinite number of sides, nor can I imagine how a polygon of an infinite number of sides could ever merge into a circle.

YALE UNIVERSITY.

ALEXANDER CAMERON.

DETERMINATE EVOLUTION.¹

I. ORGANIC SELECTION.

Admitting the possible truth of either of the current doctrines of heredity, yet there are certain defects inherent in both of them. Natural Selection, considered merely as a principle of survival, is admitted by all. It fails, however, (1) to account for the lines of progress shown in evolution where the variations supposed to have been selected were not of importance enough at first to keep alive the creatures having them (*i. e.*, were not of 'selective value'). The examination of series of fossil remains, by the paleontologists, shows structures arising with very small and insignificant beginnings.² Further, (2) in cases where correlations of structures and functions are in question, as in the case of complex animal instincts, it is difficult to see what utility the partial correlations could have had which would necessarily precede the full rise of the instinct; and yet it is impossible to believe that these correlations could have arisen by the law of variation all at once as complete functions.³ These two great objections to the 'adequacy of natural selection' are so impressive that the Neo-Darwinians have felt obliged to deal with them. The first objection may be called that from 'determinate evolution,' and the latter that from 'correlated variations.'

On the other hand the doctrine of use-inheritance or Lamarckism is open to equally grave difficulties in my opinion. (1) It is a pure assumption that any such inheritance takes place. The direct evidence is practically nothing.⁴ No unequivocal case of the inheritance of the normal effects of use or disuse has yet been cited. Again (2) it proves too much, seeing that if it actually operated as a general principle it would hinder rather than advance evolution in its higher reaches. For, first, in the more variable functions of life it would produce conflicting lines of inheritance of every degree of advantage and disadvantage, and these would very largely neutralize one another, giving a sort of functional 'panmixia' of inherited habits analogous to the panmixia of variations which arises when natural selection is not operative. Again, in cases in which the functions or acquired habits are so widespread

¹ Matter added in the foreign editions of the author's 'Mental Development in the Child and the Race.'

² Cf. the statement of this objection by Osborn, *Amer. Naturalist*, March, 1891.

³ Cf. Romanes, *Darwin and after Darwin*, II., chap. 3.

⁴ See the candid statement of Romanes, *loc. cit.*; and Morgan, *Habit and Instinct*, Chap. XIII.

and constant as to produce similar 'set' habits in the individuals, the inheritance of these habits would produce, in a relatively constant environment, such a stereotyped series of functions, of the instinctive type, that the plasticity necessary to the acquirement of new functions to any great extent would be destroyed. This type of evolution is seen in the case of certain insects which live by complex instincts; and however these instincts may have been acquired, they may yet be cited to show the sort of creatures which the free operation of use-inheritance would produce. Yet just this state of things would again militate against continued use-inheritance, as a general principle of evolution; for as instinct increases, ability to learn decreases, and so each generation would have less acquisition to hand on by heredity. So use-inheritance would very soon run itself out. Further, (3) the main criticism of the principle of natural selection cited above from the paleontologists, *i. e.*, that from 'determinate evolution,' is not met by use-inheritance; since the determinate lines of evolution are frequently, as in the case of teeth and bony structures, in characters which in the early stages of their appearance are not modified in the direction in question, during the lifetime of the creatures which have them. And, finally, (4) if it can be shown that natural selection, which all admit to be in operation in any case, can be supplemented by any principle which will meet these objections better than that of use-inheritance, then such a principle may be considered in some degree a direct substitute for the Lamarckian factor.

There is another influence at work, I think, which is directly supplementary to natural selection, *i. e.*, *Organic Selection*.

Put very generally, this principle may be stated as follows: acquired characters, or modifications, or individual adaptations—all that we are familiar with in the earlier chapters under the term *Accommodations*—while not directly inherited, are yet influential in determining the course of evolution indirectly. For such modifications and accommodations keep certain animals alive, in this way screen the variations which they represent from the action of natural selection, and so allow new variations in the same directions to arise in the next and following generations; while variations in other directions are not thus kept alive and so are lost. The species will therefore make progress in the same directions as those first marked out by the acquired modifications, and will gradually 'pick up,' by congenital variation, the same characters which were at first only individually acquired. The result will be the same, as to these characters, as if they had been directly inherited, and the appearance of such heredity in these cases,

at least, will be fully explained. While the long continued operation of the principle will account for 'determinate' lines of change.

This principle comes to mediate to a considerable degree between the two rival theories, since it goes far to meet the objections to both of them. In the first place, the two great objections as stated above to the ordinary Natural Selection theory are met by it. (1) The 'determinate' direction in the evolution is secured by the indirect directive influence of Organic Selection, in all cases in which the direction which phylogenetic evolution takes is the same as that which was taken by individual modifications in earlier generations. For where the variations in the early stages of the character in question were not of selective value, there we may suppose the individual accommodations have supplemented them and so kept them in existence. An instance is seen in the fact that young chicks and ducks which have no instinct to take up water when they see it,¹ and would perish if dependent upon the congenital variations which they have, nevertheless imitate the mother fowl, and, thus by supplementing their congenital equipment, are so kept alive. In other fowls the drinking instinct has gone on to perfection and become self-acting. Here the accommodation secured by imitation saves the species—apart from their getting water at first accidentally—and directs its future development. Farther (2) in cases of 'correlated variations'—the second objection urged above to the exclusive operation of Natural Selection—the same influence of Organic Selection is seen. For the variations which are not adequate at first, or are only partially correlated, are supplemented by the adaptations which the creature makes, and so the species has the time to perfect its inadequate congenital mechanism. On this hypothesis it is no longer an objection to the general origin of complex instincts without use-inheritance, that these complex correlations could not have come into existence all at once; since this principle gives the species time to accumulate and perfect its organization of them.

Similarly, the objections cited above to the theory of use-inheritance can not be brought against Organic Selection. In the first place (1) the more trivial and varied experiences of individuals—such as bodily mutilations, etc.—which it is not desirable to inherit, whether good or bad in themselves, would not be perpetuated in the development of the race, since organic selection would set a premium only on the variations which were important enough to be of some material use or others which were correlated with them. These being of

¹ See Morgan, *Habit and Instinct*, pp. 44 f. and his citations from Eimer, Spalding, and Mills.

such importance, the species would accumulate the variations necessary to them, and the individuals would be relieved of the necessity of making the private adaptations over again in each generation. Again (2) there would be no tendency to the exclusive production of reflexes, as would be the case under use-inheritance; since in cases in which the continued accomplishment of a function by individual accommodation was of greater utility than its accomplishment by reflexes or instinct—in these cases the former way will be perpetuated by natural selection. In the case of intelligent adaptations, for example, the increase of the intelligence with the nervous plasticity which it requires is of the greatest importance; we find that creatures having intelligence continue to acquire their adaptations intelligently with the minimum of instinctive equipment.¹ There is thus a constant interplay between instinct and accommodation, as the emergencies of the environment require the survival of one type of function or the other. This is illustrated by the fact that in creatures of intelligence we find sometimes both the instinctive and also the intelligent performance of the same function; each serving a separate utility.²

(3). The remaining objection—and it holds equally of both the current views—is that arising from the cases of structures which begin in a very small way with no apparent utility, such as the bony protuberances in places where horns afterwards develop, and in certain small changes in the evolution of mammalian teeth; which afterwards progress regularly from one generation to another until they become of some utility. While it is not clear that Organic Selection completely accounts for these cases, yet it is quite possible that it aids us in the matter; for the assumption is admissible that in their small beginnings these characters were correlated with useful functions or variations, and that the Organic or Natural Selection of the latter in a progressive way has secured the accumulation of these characters also. The facts of correlation are so little known, while yet the correlation itself is so universal, that no dogmatism is justified on either side; the less, perhaps on the side of the paleontologists who assert that these cases can not be explained by Natural Selection even when supplemented by Organic Selection; for when we enquire into the state of the evidence for the so-called ‘determinate variations’ which are supposed in these cases, we find that it is very precarious.³

¹ Groos (*Die Spiele der Thiere*, p. 65 f.) has pointed out the function of imitation as aiding the growth of intelligence with the breaking up of instincts under the operation of natural selection.

² Baldwin, *Science*, Apl. 10, 1896.

³ For example, the only way to establish ‘determinate variations’ would

We come to the view, therefore, that evolution from generation to generation has probably proceeded by the operation of Natural Selection upon variations with the assistance of the Organic Selection of co-incident¹ variations (*i. e.*, those which reproduce congenitally the acquisitions of the individuals). And we derive a view of the relation of ontogeny to phylogeny all through the animal series. All the influences which work to assist the animal to make adaptations or accommodations will unite to give directive determination to the course of evolution. These influences we may call 'orthoplastic' or directive influences. And the general fact that evolution has a directive determination through organic selection we may call 'Orthoplasmy.'²

As to detailed evidence of the action of Organic Selection, this is not the place to present it. It is well-nigh coextensive however with that for Natural Selection; for the cases where natural selection operates to preserve creatures because they adapt themselves to their environment are everywhere to be seen, and in all such cases Organic Selection is operative. Positive evidence in the shape of cases is however to be found in the papers of the writer and others on the subject.³

be to examine all the individuals of a given generation in respect to a given quality, and compare their mean with *the mean of their parents—not with the mean of all the individuals of the earlier generation*. For some influence, such as Organic Selection, might have preserved only a remnant of the earlier generation, and in this way the mean of the variations of the following generation may be shifted and give the appearance of being determinate, while the variations themselves remain indeterminate. And again, the paleontologists have no means of saying how old one of these fossil creatures had to be in order to develop the character in question. It may be that a certain age was necessary and that the variations which he finds lacking would have existed if their possessors had not fallen by natural selection before they were old enough to develop this character and deposit it with their bones.

¹A term suggested by Professor Lloyd Morgan.

²These terms are akin to 'orthogenic' and 'orthogenesis' used by Eimer (*Verh. der Deutsch. Zool. Gesell.*, 1895); his terms are not adopted by me however, for the exact meaning given above, since Eimer's view directly implicates use-inheritance and 'determinate variations' which are here rejected. On the use of these and other terms see *Science*, Apl. 23, and *Nature*, Apl. 15, 1897.

³It may be in place to recall something of the history of this suggestion as to Organic Selection and cite some of the publications bearing upon it. The present writer indicated it (only) in the first edition of this work (Feb. 1895), presented it fully with especial reference to the origin of instinct in *Science*, March 20, 1896, and developed it in many of its applications in an article entitled 'A New Factor in Evolution,' *American Naturalist*, June and July, 1896 (reprinted in *Princeton Contrib. to Psychology*, I., 4, September, 1896). Professor H. F. Osborn expressed similar views briefly in an abstract in *Science*, April 3, 1896, p. 530; and more fully in *Science*, November 27, 1896.

II. THE DIRECTIVE FACTOR.

We have now seen some reason for the reproduction of individual or ontogenetic accommodations in race progress. The truth of Organic Selection is quite distinct, of course, from the truth of any particular doctrine as to how the accommodations in the life of the individual are effected; it may be that there are as many ways of doing this as the usual language of daily life implies, *i. e.*, mechanical, nervous, intelligent, etc.

Yet when we come to weigh the conclusions to which our earlier discussions have brought us, and remember that the type of reaction, which is everywhere present in the individual's accommodation, is the 'circular reaction' working by functional selection from over-produced movements, we see where a real orthoplastic influence in biological progress lies. The individuals accommodate by such functional selection from over-produced movements; this keeps them alive while others die; the variations which are represented in them are thus kept in existence, and further variations are allowed in the same direction. This goes on until the accumulated variations become independent of the process of individual accommodation, as congenital instincts. Thus are added to the acquisitions of the species the accommodations secured by the individuals. So race progress shows a series of adaptations which corresponds to the series of individual accommodations.

It may be remarked also that when the intelligence has reached considerable development, as in the case of man, it will outrank all other means of individual accommodation. In Intelligence and Will (as will appear below)¹ the circular form of reaction becomes highly developed, and the result then is that the intelligence and the social life which it makes possible so far control the acquisitions of life as greatly to limit the action of natural selection as a law of evolution. This may be merely indicated here; the additional note below will take the subject further in the treatment of what then becomes the means of transmission from generation to generation, a form of handing down which, in contrast with physical heredity, we may call 'Social Heredity.'

Professor C. Lloyd Morgan also printed similar views, *Science*, November 20, 1896, and in his book, *Habit and Instinct*, November, 1896. The essential position was reached independently by each of these writers and has been developed by correspondence since their first publication of it.

¹ *I. e.* in the volume, Chaps. X. to XIII.

III. INTELLIGENT DIRECTION AND SOCIAL PROGRESS.

The view of biological evolution already brought out has led us to the opinion that the accommodations secured by the individuals of a species are the determining factor in the progress which the species makes, since, although we can not hold that these accommodations, or the modifications which are effected by them, are directly inherited from father to son, nevertheless by the working of Organic Selection with the subsequent accumulation of coincident variations the course of biological evolution is directed in the channels first marked out by individual adaptations. The means of accommodation were called above orthoplastic influences in view of the directive trend which they give to the progress of the species.

It was also intimated, in the earlier section, that when the intelligence once comes to play an important part in the accommodations of the individuals, then we should expect that it would be the controlling factor in race-progress. This happens in two ways which may now allow of brief statement.

1. The intelligence represents the highest and most specialized form of accommodation by 'circular reaction.' With it goes, on the active side, the great fact of volition which springs directly out of the imitative instinct of the child. It therefore becomes the goal of organic fitness to secure the best intelligence. On the organic side, intelligence is correlated with plasticity in brain structure. Thinking and willing stand for the opposite of that fixity of structure and directness of reaction which characterize the life of instinct. Progress in intelligence, therefore, represents readiness for much acquisition, together with very little congenital instinctive equipment.

It is easy to see the effects of this. The intelligence secures the widest possible range of personal adaptations, and by so doing widens the sphere of Organic Selection, so that the creature which thinks has a general screen from the action of natural selection. The struggle for existence, depending upon the physical qualities which the animals rely on, is largely done away with.

This means that with the growth of intelligence, creatures free themselves more and more from Natural Selection. Variations of a physical kind come to have within limits an equal chance to survive. Progress then depends on the one kind of variation which represents improved intelligence—variations in brain structure with the organic correlations which favor them—more than on other kinds.

2. The other consideration tends in the same direction. With

the intelligence comes the growth of sentiment, especially the great class of Social Sentiments, and their outcome the ethical and religious sentiments. We have seen in earlier chapters how the sense of personality or self, which is the kernel of intelligent growth involves the social environment and reflects it. Now this social sense also acts wherever it exists, as an 'orthoplastic' influence—a directive influence, through Organic Selection, upon the course of evolution. In the animal world it is of importance enough to have been seized upon and made instinctive: animal association acts to screen certain groups of creatures from the operation of Natural Selection.

In man the social sentiment keeps pace with his intelligence, and so enables him again to discount natural selection by coöperation with his brethren. From childhood up the individual is screened from the physical evils of the world by his fellows. So another reason appears for considering the course of evolution to be now dominated by the intelligence.

But, it may be asked, does not this render progress impossible, seeing that it is only through the operation of Natural Selection upon variations—even allowing for Organic Selection—that progress depends? This may be answered in the affirmative, so far as progress by physical heredity is concerned. Not only do we not find such progress, but the researches of Galton, Weismann and others show that there is probably little or no progress, even in intelligence, from father to son. The great man who comes as a variation does not have sons as great as he. Inter-marriage keeps the level of intelligent endowment at a relatively stable quantity, by what Galton has called 'regression.'

Yet there is progress of another kind. With intelligence comes educability. Each generation is educated in the acquisitions of earlier generations. There is in every community a greater or less mass of so-called 'Tradition' which is handed down with constant increments, from one generation to another. The young creature grows up into this tradition by the process of imitative absorption which has been called above 'Social Heredity.'¹ This directly takes the place of physical heredity as a means of transmission of many of the acquisitions which are at first the result of private intelligence, and tends to free the species from the dependence upon variations—except intellectual variations—just as the general growth of intelligence and sentiment tends to free it from the law of natural selection.

¹P. 361 and 364 (as in the first edition). See article on 'Consciousness and Evolution,' *Science*, August 23, 1895, reprinted with discussion by Prof. E. D. Cope and the writer in the *Amer. Naturalist*, Nos. from April to July, 1896.

These general truths can not be expanded here; they belong to the theory of social evolution. Yet they should be noted for certain reasons which are pertinent to our general topic, and which I may briefly mention.

First, it should be said that this progress in emancipation from the operation of natural selection and from dependence upon variations, is not limited to human life. It arises from the operation of the principle which has all the while given direction to organic evolution; the principle that individual accommodations set the direction of evolution, by what is called Organic Selection. It is only a widening of the sphere of accommodation in the way which is called intelligent, with its accompanying tendency to social life, that has produced the deflection of the stream which is so marked in human development. And as to the existence of 'Tradition' and 'Social Heredity' among animals, recent biological research and observations are emphasizing them both. Wallace and Hudson have pointed out the great importance of imitation in carrying on the habits of certain species; Weismann shows the importance of tradition as against Spencer's claim that mental gains are inherited; Lloyd Morgan has observed in great detail the action of social heredity in actually keeping young fowls alive and so allowing the perpetuation of the species, and Wesley Mills has shown the imperfection of instinct in many cases with the accompanying dependence of the creatures upon social, imitative and intelligent action.

Second, it gives a transition from animal to human organization, and from biological to social evolution, which does not involve a break in the chain of influences already present in all the development of life.

J. MARK BALDWIN.

DISCUSSION.

PROFESSOR LADD AND THE PRESIDENT'S ADDRESS.

In his discussion of my late address before the American Psychological Association, Professor Ladd makes three definite criticisms, viz.,

1. That I misunderstand Professor Wundt's position as to the nature and functions of the mind.

2. That I confound his own earlier and later books, and thus seem to find inconsistency where it does not exist.

3. That I unjustly place him in the same category with Kant, when he (Professor Ladd) claims that "we do know reality," and that "all knowledge is *quoad* knowledge, essentially transcendent" (PSYCHOLOGICAL REVIEW, March, 1897, pp. 180-182).

Regarding criticisms 1 and 3 I shall say little. It appears to me that Wundt's later utterances justify what I have said. I referred in my address to the last edition of the *Grundzüge* and to the lectures on 'Human and Animal Psychology.' As to classing Professor Ladd with the noumenalists, I think that is no injustice. Kant himself kept talking about noumena as though he knew enough about them to at least talk intelligently upon the subject and to contrast them with phenomena. Had he been quite consistent in denying us any knowledge whatever of noumena, I think he would have dropped the subject altogether. Moreover, I have nowhere charged Professor Ladd with being a good Kantian, but I think he is enough like Kant and a number of others whom I would call Noumenalists, to be properly described by the use of that term. He contrasts 'phenomena' with 'reality.' He holds (sometimes) to a reality which is not phenomenal. It matters little whether we accept his term, or that which Kant has made familiar to us, he (sometimes) treats this something, I believe, in a distinctly 'noumenal' way. He does not treat it in precisely the same way in all his books, being, as I indicated in my address, less of a noumenalist in his later works than he was in his earlier. I shall give two or three references to prove this later.

As to the second criticism made by Professor Ladd, in which he states that I have been guilty of the 'quite indefensible misapprehension' of confounding his earlier and his later works, and thus of doing him a certain injustice, I will speak a little more at length.

Professor Ladd has quite misunderstood my reference to his earlier and his later works. It would never have occurred to me to thus characterize two books published in the same year (1895), even though the preface of one of them bore the date of the year before. For all I know to the contrary, that preface may have been dated on the last day of the year, and the preface of the other on the first day of the year following. By Professor Ladd's earlier works I meant his 'Elements of Physiological Psychology,' published in 1887, and his 'Introduction to Philosophy' published in 1890. It surprises me that Professor Ladd should have fallen into error upon this point, for in his discussion of my address at the time when it was delivered, he referred to these earlier works, and in my response I stated that I had read them, and that, in comparing his later works with them, it appeared to me that he had undergone a change of mind. I still think that an examination of these works will show that he has undergone such a change.

And as I did not arrive at the opinion that Professor Ladd has modified his views, by comparing his 'Psychology' with his 'Philosophy of Mind,' so also I did not infer his inconsistency from an illegitimate comparison of the statements made in the two volumes, leaving out of consideration the difference in their aim. To prove this, let me take a single volume. An examination of the eleven references that I have made to his 'Psychology' will reveal that, in that one book, he is:

1. A Noumenalist: pp. 215, 513 and 511-517.
2. An adherent of the doctrine that mind is a self-activity within consciousness—a doctrine akin to that of the Neo-Kantians: pp. 532, 638.
3. An Empiricist, who holds that all objects of knowledge, including the self, are, psychologically considered, states of consciousness or psychoses; and that the self in consciousness does not come into being until consciousness has attained a considerable development: pp. 508, 509, 510, 519, 523, 531 and 532.

Thus the self is, according to this one book, at one and the same time the object of a metaphysical faith, an activity in consciousness, and an empirical psychosis.

As to Professor Ladd's change of faith, I will ask the reader to compare the statements of his earlier books with the two upon which I dwelt in my address. Let him, for example, read Professor Ladd's criticism of Metaphysics on page 611 of the 'Elements of Physiological Psychology' (1887). He there criticizes Metaphysics on the

ground that "it has often declared that we have an immediate and indubitable knowledge of the mind as one and the same real being in all acts of consciousness," maintaining that "*consciousness carries with it no immediate knowledge of any real and self-identical being*—not even of that real being which we call Mind, and, with good reason, assume to exist as the ground or permanent subject of mental phenomena." He states that Metaphysics treats of those *assumptions* that underlie all of our experience with what we call reality, and he draws a parallel between the hypothetical beings called atoms, which we assume to account for physical phenomena, and the real unit-being called the Mind.

In his 'Introduction to Philosophy' (1890) Professor Ladd regards 'knowledge' as the presence in consciousness of certain complexes of mental elements accompanied by a belief (pp. 230, 234, 235) or persuasion (p. 237) or conviction (p. 230) that there exists beyond consciousness (pp. 204, 225, 251) a something called 'reality' in relation to them (chapters VIII. and IX. *passim*). We get reality as an inference from experience (pp. 224, 233), and this inference is not rational but 'blind' (pp. 234, 235, 247, 251) and 'instinctive' (p. 251). It is true that in the same volume Professor Ladd, in speaking of the knowledge of the self, uses the verb 'to know' as synonymous with 'to be conscious of' (p. 226), but I think that is only a slip. The general argument of the volume is to the effect that reality is something that I think I may justly call noumenal, and not something immediately known.

In the two books to which I made so many references in my address, Professor Ladd finds the reality of the self to be involved in every act of knowledge 'as an immediate datum of experience,' and he no longer describes the metaphysical faith which gives us reality as 'blind;' as a Neo-Kantian, he makes the real self a self-activity in consciousness; as an empiricist, he makes it an empirical psychosis. I think I have not been wrong in believing that he has modified his views.

I cannot help thinking the tone of Professor Ladd's communication a trifle sour. He speaks of my criticism as 'raillery,' and intimates that I have dealt with the works of various philosophical writers in a spirit of levity. I have carefully re-read what I have written and I cannot see that it is not courteous and in sufficiently good taste. My address contained but one jest, and that one was borrowed from Professor Ladd himself and merely adapted. If it has annoyed him I of course regret having used it, for it is no part of the work of a critic to

needlessly hurt the feelings of the person criticised. I wrote with all seriousness. I believe that Professor Ladd's utterances are conflicting, and it seems to me that anyone who points out this fact does him a real service.

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VISCERAL DISEASE AND PAIN.

In a series of papers published between 1893 and 1896, Dr. Henry Head has treated 'Disturbances of Sensation with Especial Reference to the Pain of Visceral Disease.'¹ The starting point of his investigation is the well-known fact that visceral disorders are frequently accompanied by cutaneous tenderness, the pain occasioned by organic disturbance being 'referred' by the patient to an area on the surface of the body. Dr. Head has carefully mapped out these areas, designating in his first paper those which lie below the first dorsal segment, and in his second paper those which are found on the head and neck. His third paper deals, not with the topography of the areas, but with the pain caused by diseases of various organs. His report contains a vast amount of clinical evidence interspersed with theoretical considerations. The value of his contributions to pathology, anatomy and physiology has been duly recognized. And, quite naturally, the results which he obtained and which certainly throw light on an intricate problem, have been pressed, with some eagerness, into the service of psychological theory.² Whether there are separate nerves for pain is a question which cannot be fully discussed here; but the evidence in favor of the affirmative view supplied by Head's research is such as to deserve examination.

It seems to be fairly established that in cases of visceral disease certain skin-areas are affected in such a way that they show increased tenderness, increased reflexes, and one or more maximal points to which the pain is referred and to which the tenderness is limited as the disturbance subsides. The tenderness can be tested by applying a pin to the sensitive area, in which case the rounded head causes as sharp a sensation as the point causes on normal surfaces, while the application of the point gives rise to excessive pain. Quantitative data as to the amount or duration of pressure are not furnished in Dr. Head's report.

¹*Brain*, XVI., 1893, p. 1; XVII., 1894, p. 339; XIX., 1896, p. 153.

²*Pain Nerves*. Herbert Nichols, *Psy. Rev.*, May, 1896, p. 309.

What is, perhaps, of greater importance, he has shown that the eruptions in *Herpes Zoster* occupy areas which have the same distribution and the same maxima as the areas of tenderness in visceral disease.

From these statements it must not be inferred that the topography of the areas of tenderness is a simple or easy affair. At times only the maxima can be determined; in nearly all cases more than one area can be pointed out; and certain areas of the body which are rarely affected, appear, when they do become tender, in combination with others.

With these facts as a basis, Dr. Head proceeds by way of elimination to show the significance of the areas. They bear no relation to cortical distribution, nor do they correspond to the distribution of peripheral nerves. Do they represent the supply from posterior nerve-roots? To this question a negative reply is given. The areas supplied from the roots overlap, whereas the areas of cutaneous tenderness and of herpetic eruption do not overlap. Hence the inference that each of these latter areas represents the supply from a single segment of the cord. And since the touch nerves issue from several segments and, in their distribution, overlap, it would seem to follow that there are separate paths for touch and for pain.

So much stress is laid, in deductions of this sort, upon the way in which the zones are mapped out, that one may be permitted to look more closely at the facts of distribution. To begin with, it must be noted that the proofs for the overlapping of the touch-areas and the proofs for the limitation of the pain-areas, are not of precisely the same character. Sherrington found that when, in the monkey, a single posterior root is divided, there is no absence of sensation; and Head, in some few cases, observed the same thing in man. The same area, therefore, must be supplied from several roots and fibres from these must interlace. But in mapping out the areas of tenderness, Head observed a large number of subjects, locating an area in this patient and another area in another patient and so on. His criterion is this: "If they overlapped to any considerable extent, like the areas of common sensation, the extent of skin covered when any one was present must necessarily be greater than that left unaffected when the areas on each side of it were tender. That is to say—supposing Nos. 1 and 3 were tender, the skin between their borders, which was unaffected, must of necessity be of smaller extent than that affected when No. 2 only was tender." Whatever be the accuracy of this method, it is obviously less direct than the method employed to demonstrate the overlapping of touch-zones. Dr. Head himself does not contend for an absolute definition of the areas of tenderness; he admits more

than once that there is some overlapping, though this is slight as compared with the overlapping of zones supplied from the posterior roots. It is a 'difference of degree.' Another investigator, Dr. Mackenzie, is more emphatic. He tells us: "From the study of cases of *Herpes Zoster* and of the hyperæsthetic areas associated with visceral disease, I have come to the conclusion that there is distinct overlapping of the fields of cutaneous supply in individual nerve roots, of pathic, thermic and trophic fibres as well as of those of ordinary sensation."¹ This divergence suggests that further investigation may be necessary before the argument from overlapping as against sharp definition can be securely formulated.

Much depends, of course, upon what is meant by 'different areas of distribution.' Experiment has shown that on the skin there are points for heat and points for cold; whence it is reasonable to infer that there are separate paths for these two kinds of stimulation. If the areas in question were simply expansions of similar points, each having its special and exclusive function, the evidence in favor of separate nerves for touch and for pain would be strong. There might be irregularities in the distribution and different degrees of sensibility in the various areas; but once the work of mapping out had been accomplished, we would be able to indicate, for any given area, its particular function. It will hardly be claimed that our topography of the skin has attained this ideal accuracy, so far as zones for pain distinct from touch-zones are concerned. In particular, the results published by Dr. Head do not establish any such clear demarcation. Consequently, it is not in this strict sense that Dr. Nichols must be understood when he states as a fact that "the zones of distribution for pain, heat and trophic nerves cover markedly different fixed areas of the skin from the zones of distribution of the touch-nerves."

It is possible that we are exacting too much—insisting on proofs that will never be forthcoming. In fact, different areas of distribution may be conceived after a less rigorous fashion. Different functions might occupy in part the same area, though their respective zones have different boundaries. It might be shown, for instance, that in a total area which we will call 12, the zones 1-9 are sensible to tactile stimuli and the zones 3-12 are sensible to painful stimuli. In this case, we should say that the zones for touch overlap the zones for pain, or vice versa, without admitting that one touch-zone overlaps another touch-zone, or that the zones for pain overlap one another. Under such conditions, the argument for separate pain nerves would be a more

¹ *Brain*, XVI., 1893, p. 349.

labored one. Nevertheless, it would have a weight of its own—provided that areas of this sort could be marked off on the normal subject.

There is more reason for doubt where difference of distribution hinges upon a change from normal to abnormal conditions. The argument might then take on several forms, one of which may be mentioned simply to show that the phrase 'markedly different fixed areas' needs careful interpretation. From the diagrams furnished by Dr. Head, it appears that the areas of cutaneous tenderness in visceral disease are quite large, extending in some cases in broad bands around the body or along the limbs. If the 'fixedness' could possibly imply that these areas are in all cases, normal no less than pathological, reserved for pain, the markedly different areas for touch would be rather limited. The likelihood of such a misconception is not great where one merely compares the normal condition of any organ with its abnormal condition. But at present we are dealing not with local, but with referred pain. The disease is visceral; the skin is supposed to be normal—or at least to be affected in only a roundabout way. Accordingly, one might infer, in consequence of the markedly different areas, that considerable portions of the skin are set apart for painful stimulation, and that they enter upon this function when the necessary condition, visceral disorder, is realized.

A more plausible form of the argument: areas supplied from the posterior roots and serving the function of touch overlap in normal conditions, whereas, in visceral disease, areas are marked off which serve the function of pain and do not overlap. This brings us in view of the question whether the same fibres which, under normal conditions transmit tactile stimulation, do not serve as pain-paths in visceral disease. It will be remembered that, according to Obersteiner's observations, there are diseases in which tactile stimulation of one portion of the skin gives rise to a sensation which the patient localizes in another portion. This allocheiria is due to a lesion in the central nervous system. According to Dr. Head, "the phenomena of allocheiria and of referred pain in visceral disease are in nature and explanation essentially the same. Both depend for their appearance upon the law that where a painful stimulus is applied to a part of lower sensibility, the pain produced is felt in the part of much greater sensibility, the part of lower sensibility to which the stimulus was actually applied." This explanation bears directly upon the transferred localization of tactile and painful stimuli; but it will also account, I think, for the painful feeling itself. As a result of visceral disease there is a height-

ened excitability at the point of central connection; hence the exaggerated reflexes which characterize the affected skin-areas. A stimulus which, under normal conditions, would produce only a sensation of touch, passes into the modified center and is referred, in painful phase, to the stimulated area. Or again, stimuli originating in an internal organ and producing ordinarily unconscious reflexes, are referred, in the altered condition of the cord, either to a superficial area or to the diseased region itself, as is the case when the serous cavities of the body are affected. On this hypothesis, the difference between parts of higher sensibility and parts of lower sensibility might, to some extent, be explained. The higher sensibility of superficial areas is more easily understood if we suppose the same fibres to conduct tactile and painful stimulation. Frequency of tactile stimulation and transmission would increase the sensibility of the skin areas, whereas, on the hypothesis of separate paths, it is difficult to see why the skin should be more sensible and why the pain should be referred to it rather than to the seat of disease.

Should this view prove correct, the difference of distribution would cease to be a primary factor in the problem. The effect of stimulation would depend, not so much on the number of segments in the cord that it reaches, as on the condition of any or of all the segments. Whether the areas of cutaneous tenderness in visceral disease are sharply defined or distinctly overlap, is at best an open question. Its final settlement, no doubt, will be hastened by painstaking research along the lines of Dr. Head's investigation. One may fully appreciate his work without feeling bound to declare, in the words of Dr. Nichols, that it 'must set this dispute at rest forever.'

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PSYCHOLOGICAL LITERATURE.

Analytic Psychology. G. F. STOUT. London, Swan, Sonnenschein & Co.; New York, Macmillans, 1896. 2 vols. Pp. xi+289 and 314.

There can be no hesitation in pronouncing this the most important work in general psychology by a British author since Ward's *Britannica* article of a dozen years ago. That article marked an epoch in British psychology by its complete break with the traditional Associationism: it was a proclamation of independence. Mr. Stout's work shows that the independence has been won. "It may be said," he writes, "that at present the psychological world is divided into two camps; on the one side are the champions of Association, on the other the champions of Apperception. * * * I have definitely sided with the second party" (ii., p. 41). What Oxford has done for metaphysics, that Cambridge has accomplished for psychology. And both movements, the psychological no less than the philosophical, stand evidently under the commanding though modified influence of the same man, Kant.

The 'Analytic Psychology,' however, follows, as its title indicates, the traditional English method. At the same time it suggests a contrast to another *genetic* psychology, and Mr. Stout's main interest is, he tells us, with the latter. But just as the geologist acquires knowledge of the nature of geological changes by observation of the changes that are going on now, so for investigating the origin and growth of mental products, it seemed necessary first to analyze the developed consciousness and to study the laws of mental process in present experience. In pursuing this method, Mr. Stout avoids the infelicities of an 'evolution' of mental life on the basis of imaginary 'principles of psychology,' and succeeds in giving a strong impression of what our mental life really is and of the principles which actually govern it, at least in those forms of it here considered. For, in the opinion of the author, some products of mental life can be more profitably studied from the point of view of their development, and their consideration is accordingly reserved for a future work. The number of topics omitted in the present work is certainly striking, but judgment on the special wisdom of the omissions may be deferred till the promised 'Genetic Psychology' is also before us for comparison.

The general plan of this treatise is as follows: An introduction on the scope and method of psychology is followed by two books, the first of which contains the general analysis, the second a more detailed examination of processes. Book I., after discussing the principle for the division of ultimate mental functions (Chap. I.) and the possibility of their analysis (Chap. II.), distinguishes the fundamental forms of the cognitive consciousness (Chaps. III.-V.) and concludes with a chapter on feeling and conation (Chap. VI.). Book II. follows a similar arrangement. Beginning with a discussion of the conception of mental activity (Chap. I.), it then examines, in a general synthetic order, the cognitive processes (Chaps. II.-XI.) and concludes with a chapter on pleasure and pain (Chap. XII.)

Psychology is defined as 'the positive science of mental process' (p. 1), including mental development (p. 9), in individuals (p. 7). Its data are distinguished as (1) products of past process, (2) the process itself as introspectively and retrospectively observed and (3) certain external signs. Specially valuable among the first is the material furnished by philology and anthropology; Mr. Stout thinks that the contributions from these sources may ultimately prove of at least as much importance for psychology as those yielded by physiology. Of greater interest is the author's adoption of the hypothesis of 'psychical dispositions' as a means of connecting present conscious process with the results of conscious process in the past. This conception controls the whole of his psychology. He considers it and, indeed, shows it to be distinctly preferable to the hypothesis of sub-consciousness and more practicable than the corresponding physiological hypothesis. Our ignorance of the precise correlation of mental process and physiological process is such, he says, that physiology cannot be made the sole basis of psychology. Under certain assignable conditions, the two sciences might be merged in one; but the realization of those conditions appears at present infinitely remote. Even when it is recognized that a 'psychical' disposition is a 'physiological' disposition also, it is still very often necessary for the sake of clearness to separate the purely psychological side of the process from corresponding physiological data and hypotheses.

As a positive principle for the division of ultimate mental functions, Mr. Stout adopts Brentano's—the mode in which consciousness refers to an object; but he criticizes Brentano's use of it, especially in identifying the 'object' with the immediate conscious content. According to Mr. Stout, there is present in all 'noetic' experience, over and above the presentation as modification of the individual conscious-

ness, a unique thought-reference to something which, as the thinker means or intends it, is not a present modification of his individual consciousness. "The object of thought is never a content of our finite consciousness" (p. 45). It is difficult to follow Mr. Stout here. The above statement, for example, taken literally, would seem to make psychology itself impossible. This, of course, is not meant. "The point is that the object as we mean or intend it, cannot be a modification of our consciousness at the time we mean or intend it" (p. 46). But is this really so? It is true that the process of cognition is distinct from its object, but it does not follow that the object is not immanent. Mr. Stout says, indeed, that in thinking of a sensation, I qualify it, as an event in my mental history, by reference to other experience not present, and that in considering abstractly a content as such, I generalize it, regard it as one of an indefinite series. But clearly, if for psychological purposes I attend to a visual appearance, as such, whatever reference to an 'external' object or to other portions of my experience may be implied, what I mean and intend is not those objects but just this present modification of my visual experience. It may be said, perhaps, that a modification of consciousness is continually changing, and that to be conscious of it, I must be conscious of it as a process, but that the parts of a process cannot possibly be all present together, and that consequently in grasping the unity of its successive phases, I necessarily transcend the immediate present. The reply to this is, that there is no evidence that modifications of consciousness form a succession of timeless instants. What we mean by a present modification of consciousness is a modification in the 'specious' present. The evidence has yet to be given that a present content of consciousness cannot be an object of thought while it and the process of attending to it lasts.

In the second chapter, the theoretical objection against the possibility of analyzing presentations, viz.: that a discriminated content cannot be identical with one that is undiscriminated, is met by the rejoinder of irrelevancy; it is not necessary that the two contents should be identical, but only that the undistinguished differences present in the original experience should be adequately represented by the analytic distinctions in the new. A similar explanation is given of the analysis of dispositions. Here, to be sure, the discovered distinctions do not actually exist prior to their discovery; they are, however, determined by a mental condition other than the process of fixing attention.

Sentientia as a mode of consciousness was briefly referred to in the

general analysis of 'noetic' experience in Chapter I.; but this side of experience receives scant consideration in the present treatise. Mere sentiency would be 'anoetic.' Chapters III. and IV. deal with modes of simple apprehension. Emphasis is placed on the apprehension of form of combination, corresponding to the German '*Gestaltqualität*,' as a unique mode of consciousness distinct from the apprehension of the matter and from the apprehension of relations, both of which presuppose it. Besides these modes of explicit apprehension, there are modes of implicit apprehension, which appear in all cases of 'psychic fringe' and one special case of which is that mental state we call understanding the meaning of a word. Mr. Stout's admirable discussion at this point forcibly illustrates the picturesque remark of Professor James in a similar connection that "introspective psychology must here throw up the sponge." Stout himself falls back on unconscious mental process.

Chapter V. follows Brentano in treating judging or believing as distinct from simple apprehension. The expression 'judging or believing' is misleading in that it suggests that the two are identical, and the comment on it on p. 98 is not, we think, altogether happy. However, the point is that judging, as implying belief, is a unique attitude of consciousness towards objects. Mr. Stout calls it 'the Yes-No consciousness.' Might we not, perhaps, call belief the psychical modality of judgment? Certainly, apart from emotional coloring, degrees of assurance seem to be, as Mr. Stout says, 'degrees of firmness or fixity rather than of intensity' (p. 110).

The cognitive consciousness has thus been analyzed into the three fundamental modes of sentience, simple apprehension and belief. Chapter VI. analyzes the volitional consciousness into feeling (pleasure or displeasure) and conation (desire or aversion). Specially noteworthy is the treatment of striving in 'noetic' consciousness as a mode of attention, the two being distinguished in dynamic reference only as the direction of mental activity to an end is distinguished from the activity itself in the successive phases of its realization (p. 126). From this point of view aversion is regarded as attention constrained.

The Second Book opens with an explanation of the conception of mental activity. Accepting Bradley's view that 'activity' implies a self-determined process in time, Mr. Stout finds physical analogues for the psychological conception in movement under the law of inertia, where the continued motion of a body is traceable to its own previous motion, but particularly in the reactions designated by Avenarius *vital series*, where the process not merely perpetuates

itself, but adapts itself to an end, and is directly and indirectly self-developing. The analogue is most striking in the central nervous system, where the physical process is actually correlated with the mental. The proof that the mental process is self-determining is (1) that it initiates the changes on which its propagation immediately depends, and (2) that the brain-substance in which these changes take place has been rendered capable of them only through previous psychophysical process in which it has taken part. The fact that its self-determination is indirect is no reason for regarding it as a fiction. In the sense, therefore, in which 'activity' can be referred to physical process, it can be referred to mental process. The point in which all physical analogies fail is that the mental process feels its own current. James, Baldwin and Bradley are wrong in identifying the activity of consciousness with certain selected aspects of the process. The distinction between its passivity and its activity is relative. The whole process is active. Mr. Stout seems at times to say that we have an immediate experience of its degrees (see, *e. g.*, pp. 160 f). He finds no meaning in the attempt to *place* the feeling-aspect of the consciousness in organic or muscular sensations. But suppose the question is put in this form: Could a disembodied spirit actually *feel* his conscious life as distinguished from being conscious of it?

The special analysis of mental process takes up, first, (Chaps. II., III.) attention, which is regarded, not as a 'special activity,' but as a process coincident with noetic consciousness generally. The treatment is masterly from every point of view. It has the prevision, the sureness of touch, the finish of a skilful demonstration in anatomy or, let us say, of a performance by a great artist on the violin. Stress is laid on the systematic complexity of the process, on its character as a prospective attitude, on its relation to mental development, especially in its dependence on preformed dispositions. Its teleological aspect—its tendency to go on until the end is reached and then to stop—is excellently considered, as is also its inhibitive aspect, for which a purely psychological explanation is found particularly in the systematic unity of the process and its relation to preformed dispositions. As to the physiological correlate of attention, some such conception as that of higher and lower level centres (Huglings Jackson) is preferred to that of special centres of attention (Wundt) as corresponding more closely to the features of the psychological process. Wundt's postulate rests on the grave psychological error of separating the activity of attention from its content. Among other points of interest in the chapter are the conception of interest as the hedonic aspect of atten-

tion (p. 225), the careful discussion (pp. 225-236) as to whether attention is ever determined by pleasure and pain, as such—which is seriously doubted—and the refutation of the other common opinion that attention makes its object clearer and more intense (pp. 244 ff). Exception may be taken to this statement or to that, but the analysis as a whole is carried through with remarkable strenuousness and consistency. It would be easy to point parallels to every single feature of the doctrine, but as here worked out, it is, we think, a distinct advance on anything that has been written on the subject hitherto in English. This is particularly to be said in view of certain applications of it in the sequel.

Chapter IV. deals with the more mechanical aspects of conscious process, retentiveness, habit and association. The well-worn subject of habit receives new light from the suggestion that the transition from volitional to automatic action is due, not merely to the effect of repetition, but also to the teleology of attention (p. 265).

Chapter V. deals with the synthesis of presentations in the reference of thought to a single object. 'Noetic synthesis' implies "the introduction of a distinct kind of mental factor, the apprehension of the whole which determines the order and apprehension of the parts" (ii., p. 41).

In Chapter VI., with explicit reference to Bradley's criticism of Associationism in his *Principles of Logic*, Mr. Stout dwells on the constructive synthesis which pervades even the lowest phases of mental process. While associationists tend to represent the whole as due exclusively to the combination of the parts, the thesis here maintained is that every new synthesis results from the further determination of parts within a pre-existing whole. The special aspect of the process treated in this chapter is 'Relative Suggestion,' *i. e.*, the continual spontaneous readaptation of already acquired experience to novel conditions. There is no such a thing as a mere 'literal resuscitation, revival or reinstatement' of former associations.

Chapter VII. on 'Conation and Cognitive Synthesis' develops the counterpart of the doctrine that all conation is attention, namely that all mental process is, as such, conation. From this point of view cognitive synthesis is regarded, not as a web which conative tendencies spin, but as a further defining and differentiation of those tendencies themselves.

Then comes the great chapter (VIII.) on Apperception in which all the preceding discussion is brought to a head. This emphasis on apperception is new in British psychology. Mr. Stout's conception

of the process is also new. He has been greatly influenced by the Herbartians and it is in Herbart's sense rather than in that of Leibniz or of Kant that he uses the term. But he differs from Herbart primarily in his conception of the preformed mental system as an organized whole involving noetic synthesis—this as opposed to the conception of a mere apperception-mass of presentations—and then in regarding the entire process as an evolution in which neither the apperipient nor the apperceived factor is at any time either exclusively passive or exclusively active. He defines it as "the process by which a mental system appropriates a new element, or otherwise receives a fresh determination" (p. 112). It expresses the growing point of mind and is a feature common to all understanding, interpreting, subsuming and the like. Among the important features of the doctrine are the conceptions of 'negative' and 'destructive' apperception, the former occurring where the effort to incorporate a new element is defeated, the latter where "one system by appropriating a new element wrests it from its preformed connection with another system." The effect, however, in either case is to develop an apperceptive system of some sort. In the case of 'negative apperception,' for instance, though the system incorporates no new element, it receives a fresh determination and the process can never be repeated under precisely the same conditions again, while as part of a more comprehensive process, it directly conditions positive mental development. Of even greater interest, if possible, is the working out of the conceptions of the coöperation and competition of apperceptive systems, of the conditions which determine their strength and of their conflict and its issue. These topics are all skilfully handled with abundance of acute observation and illustrative detail. The hypothesis of psychical dispositions formed under the influence of attention from which they derive their systematic complexity—the conception of such preformed dispositions reacting on the further process of attention thus becomes, in the hands of the author, a powerful instrument for analyzing the most intricate of mental processes, the process of mental organization and growth. Doubtless much remains to be done in exhibiting the mechanical aspects of the process, and the unity of apperception which appears as an ultimate datum of the analysis constitutes an important and difficult problem. But the thorough and comprehensive treatment of the subject here given is likely to remain for long a standard of reference. One word as to terminology. Is it necessary or desirable to speak of the process of the further determination of a content of attention as a process in which one idea, group or system

'apperceives' the idea which it appropriates or by which it is otherwise determined? We do not say that the idea of red 'perceives' the idea of hardness. The Kantian terminology is here, we think, decidedly preferable to the Herbartian because it relates 'apperception' to that consciousness of self as subject which, whether contributing anything or not to mental process, is certainly very much in evidence and moulds and colors the significance of common speech.

The chapters on 'Comparison and Conception' (Chap. IX.) and on 'Thought and Language' (Chap. X.) deal especially with the problem of the universal. Conceptual thinking is thought of the universal, as such. Psychologically the universal is the apperceptive system with its universal objective reference. The problem is, to get this into the foreground of consciousness; its solution is chiefly by comparison and by language. The great function of language is to fix and detain, and so render capable of further manipulation, apperceptive systems by means of expressive signs (p. 192). The way language does this is very carefully explained.

Chapter XI. is on 'Belief and Imagination.' Belief is regarded both as a condition of activity and as a result of the limitation of activity. An illustration of the latter principle is the belief in external reality. The brief summary of the author's controversy with Dr. Pikler on this point (pp. 245-248) leaves, however, a rather confused impression. And, as regards the former principle, while it is no doubt true that the acceptance of a proposition means that I *can* make it a starting-point or a link in a process of reasoning ultimately affecting conduct (p. 238), it is by no means clear that I always must. A large number of our theoretical beliefs, accepted on mere authority, appear to yield themselves in fact to no further theoretical uses and to have no direct bearing on conduct.

In the final chapter of the work, the author applies his general conception of mental process as activity tending to an end to the theory of 'Pleasure and Pain' (Chap. XII.). Pleasure, it is held, arises where the activity is unhindered, pain where it is for any reason thwarted or checked, the intensity of the affective state depending on the intensity and complexity of mental excitement and the degree of its hindrance. The theory is abundantly illustrated, and the first part of it, at any rate, may be regarded as fairly well made out for all cases susceptible of psychological analysis. The second part—Mr. Stout unfortunately does not make the distinction—is more doubtful, for it is obviously impossible to compare directly with any accuracy, degrees of intensity of affective states or degrees of complexity of the processes con-

cerned in them. There is, besides, a difference between intensity and amount of feeling, *e. g.*, in the pleasure of indolence as compared with some other pleasures, and this difference requires to be accounted for. In its psychological form, the theory is admittedly inadequate to account for the so-called pleasures and pains of sense. At this point, Mr. Stout translates the principle into physiological terms. Following the clue of the psychological analysis and, assuming that the tendency of mental process is correlated on the physiological side with a tendency of disturbed neural arrangements to equilibrium, the thesis is that "pleasure and pain depend respectively on the uninterrupted or interrupted course of the vital series" and that "intensity of pleasure or pain depends on the intensity and complexity of the pleasant or painful excitation." The theory is then applied to the affective states connected with various classes of sensations, Mr. Marshall and the 'nutrition' theorists coming in for a good deal of effective criticism by the way. His own theory recommends itself to Mr. Stout, in the absence of any positive knowledge of what the physiological process really is, by its comprehensiveness—it assumes that pleasure and pain are produced in all cases in the same way—and because of its basis in psychological experience. It should be noted, however, that the psychological basis is the teleology of the process of attention. The pleasures and pains of sense, on the other hand, have to do directly with 'anoetic' consciousness. And here the process may be quite different. Certainly, as Mr. Stout himself admits, the conception is quite vague when applied to cutaneous pain, especially, we may add, when its purely sensational character is admitted and even the possibility of special pain-nerves.

Though but a fragment of a larger whole, the present treatise is as complete in itself as—may we say?—Schubert's 'Unfinished Symphony.' In each case the intention of the author is completely worked out and in both the execution is finished in the highest degree. Mr. Stout elaborates his thought through all the intricacies of its movement with masterly freedom, sustained power, copious illustration and in the classic style. The book is extremely well written. Severely rigorous in analysis, fixing and defining the most subtly evanescent and baffling of phenomena, it rarely happens that the thought is not clearly expressed. It is one of the books that will live. It will take its place among the great works in the history of English psychology.

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Contributions to the Analysis of the Sensations. ERNST MACH.
Translated by C. M. WILLIAMS. Chicago, The Open Court Publishing Co. 1897. Pp. xii+208, 37 cuts. \$1.25.

In the present condition of psychological literature in English an important translation is more of a contribution than any except the best of original work, and such a contribution has certainly been made by the translation of this little book of Mach's. Its distinguishing feature is freshness of view. Instead of the glorification of physics as the ideal toward which psychology should strive which is now and then heard from psychologists themselves, Mach tells us in his preface that he is profoundly convinced "that the foundations of science as a whole and of physics in particular, await their next greatest elucidations from the side of biology and especially from the analysis of the sensations."

How this can be is made clear by the first two chapters. The first develops the general standpoint of idealistic, or, more exactly, sensational monism; the sensations are the 'elements of the world' and their interrelations the subject matter of all science—this standpoint being held, of course, not as a permanent philosophy but as a working hypothesis. The second chapter, on the Chief Point of View for the Investigation of the Senses, advocates a rigid psycho-physic parallelism—no sensation without a corresponding physical change; like sensations, like changes; if space is tridimensional, the underlying neural process will also be found threefold. Such a parallelism follows more or less naturally from the monism of the introduction.

The next three chapters are devoted to an analysis of spatial vision: the first chiefly to physiological similarity and symmetry, the second chiefly to illusions of movement, and the third to normal and illusory perceptions of perspective and the like. The first emphasizes the motor factor in visual space; the second leads up to "the will to perform movements of the eyes, or the innervation to the act," as the essence of that space; and the third offers as a tentative explanation of the phenomena treated, certain habits of the eye, largely independent of consciousness and a result of race experience, which favor seeing according to the greatest probability. Something of this kind, though very differently formulated, is at the bottom of Thiéry's recent explanation of geometrical-optical illusions, and something of the kind seems necessary to bring order into this rather confused field.

The chapter on Time which follows is less interesting—in part perhaps because of its greater difficulty and in part because Mach himself has done less original work in this field.

Sensations of tone are considered in the seventh chapter, the most important sections being those in which the author explains pitch on the basis of only two specific energies instead of the very large number often assumed, and those in which he suggests a hypothetical explanation of the positive character of harmony which musicians have generally declared that Helmholtz neglected in his theory.

The final chapter deals with the philosophy and psychology of science from the monistic standpoint of the introduction. The psychology of the acquisition of knowledge, of judgment, abstraction, concepts, natural laws, mathematical space and physical time are all briefly considered. To the text of the German edition a good number of notes, two appendices, and a full index have been added.

The book is hardly one which the general reader will master easily in all its details, but as a book in which special students who have passed the stage of the text-books and laboratory practice may make the acquaintance of some of the open questions of sensation, and, at the same time, take a lesson in the charm of scientific modesty and reasonableness, it can hardly be excelled.

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Consciousness and Biological Evolution (I, II.) The Religious Instinct. The Function of Religious Expression. H. R. MARSHALL. Four articles. *Mind*, July, 1896, to April, 1897.

The first two articles of this series, proceeding upon the assumption of a Spinozistic parallelism of the physiological and psychical, seek to set forth two correspondences, that of instinct to biological constancy and conservatism, and that of reason to biological variation in its highest aspect. As to the first point, Mr. Marshall says that instinct as lapsed intelligence means merely "that as habit becomes more fixed, neural action becomes more thoroughly organized; and that correspondingly the psychic elements coincident with the neural activities become less and less emphatic in the pulse of the preëminent consciousness with which introspection acquaints us." But Mr. Marshall does not make clear why, as neural activities are organized, 'preëminent consciousness' lapses. On the contrary, parallelism would suggest that the more organized the neurosis the more organized the psychosis, and so not its failing but strengthening. Parallelism would say that only upon the supposition that neural organization means 'less emphatic' neurosis will psychosis appear as 'less emphatic,' that is in instinct form. But this supposition is obviously untrue. Further Mr. Marshall by his definition, which he defends at length, of instinct as organized activities, and then explaining instinct

by organization simply refers instinct to itself. The question will doubtless occur to many why instinct should be restricted to conservatism. Are not 'cranks,' originals and geniuses a type proceeding from organization? Do not such tendencies run in families?

Mr. Marshall later gives an interesting but by no means conclusive account of social instinct in relation to the individualistic and specific.

As to variation Mr. Marshall emphasizes it as independent activity, 'an element of an aggregate' acting as 'isolated entity.' But while variation is obviously independent activity, it is not necessarily, as seems implied, wholly individualistic. On the contrary, variation is mainly toward the aggregate, it is the initiation of organization. Indeed, as in radical clubs, variation may be said to be organized. The general trend of variation is toward solidarity and centralization. But changeability and volatility may become so constant a characteristic of a race, *e. g.*, the French, as to be a certain kind of conservatism. That reason is in man the chief variant process hardly needs 'argument.' In Section 16 Mr. Marshall thus sums up his doctrine of variation: "The suggestion then which it seems to me biology may gain from this special psychological view in reference to the nature of variation is that organic variation is probably due, in large measure at least, to the tendency of elements in organic aggregates to react as though they were isolated entities, rather than integral parts of a complex systematized unity; acting thus whenever the force reaching them from their environment is so emphatic that it overcomes the forces inherent in the organism of which they are elements, or compels reaction before sufficient time has been allowed for these organic forces to become effective." This, in plain English, equals "variation is due to a tendency to vary." Here then, as in the case of instinct, Mr. Marshall travels in a circle.

The last two articles deal with the religious instinct and its expression as an example of biologic conservatism, the first article being a deduction of religious instinct as a necessary function to socialization, and the second article being an induction from the facts of seclusion, fastings, self-torture, initiation, prayer, sacrifice, celibacy and pilgrimage, as religious practices, that religion has actually exercised this function of restraint of individualism and promotion of sociality. It would take us much too far afield to consider these articles more closely at this time, but while they are suggestive, we think that the sketch is too summary to satisfy most readers. We hope they serve the author only as an outline for an extended research and discussion yet to appear.

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RECENT WORKS IN PHILOSOPHY.

Christianity and Idealism. By JOHN WATSON, LL.D. The Macmillan Company, New York and London. 1897. Pp. 211.

The Life of James Mc Cosh. Edited by WILLIAM MILLIGAN SLOANE. New York, Charles Scribner's sons. 1896. Pp. 287.

Professor Watson's book is the first publication, though second in the series, resulting from the laudable enterprise of the Philosophical Union of the University of California. Passing over the first part of the volume, on account of space limit, we come in the second part to the discussion of special interest to philosophical readers, that of the relation of modern Idealism to the Christian ideal of life. In his preface Professor Watson includes under the term Idealism such different systems as those of Descartes and Hegel, Kant, Spinoza and Lotze. The fundamental principle of idealism is expressed in the proposition, the real is rational. The departures of any of the above thinkers from pure idealism is to be measured by their departure from this principle. Now broadly conceived, the rationality of the real is held by many who are not accounted as idealists. But the school of idealism, with which Professor Watson is most in sympathy, tends to identify the real and the rational in the sense that reality in its last analysis reduces to the activity of thought.

Under the influence of this presumption which tends to narrow the principle of idealism to the tenet of a school the author proceeds to interpret the content of Christianity in accordance with the rational categories. But in this effort both elements are subjected to a severe strain. The central category of Christianity, whether we view its historical content or that of the living christian consciousness of the present, is, without doubt, that of concrete, personal spirit. The difficulty of Professor Watson is that of reconciling this category with the principles of a philosophy which tends to reduce the real to ultimate terms of thought. That by the application of force a species of adjustment may be effected is no doubt true. But the only satisfactory treatment of the relation would consist in such an exhibition of essential unity between the content of Christianity and the principles of Idealism as would make it appear that Christianity itself, when it becomes reflective, naturally and normally expresses itself in the terms of the idealistic creed. Now it is one thing to say that the reflective Christian consciousness will be broadly idealistic, but quite another to maintain that it will find its most adequate expression in the ready-made princi-

ples of any of the idealistic schools. Professor Watson speaks as the mouthpiece of a special form of idealism, and his is perhaps the most eloquent and persuasive voice of his school. But there are those, and among them I am forced to count myself, who are not convinced that the main contention of the author has been successful, and who believe that complete unity between Christianity and Idealism would involve more than thinkers of Professor Watson's school are willing to concede.

No one who reads Professor Watson's book will fail to be impressed by its great ability and its positive merits. It is written in the author's best style and it rests on the firm belief that the vitalest problems of philosophy are those of religion and that a philosophy which takes a negative attitude toward religion, or attempts to shirk its problems, proves recreant to its most pressing duty. Professor Watson's faith in the ultimate unity of philosophic and religious truth is also reassuring in view of the hesitating tone of so many of our thinkers. And that he has made a noble contribution to the religious thought of the time none will be more ready to admit than those who are not convinced that the specific aim of the last section of his book has been completely attained.

The life of James McCosh is mainly autobiographical, taken from notes written down by him during the last years of his life. But these notes were incomplete and at times fragmentary and the editor, Professor Sloane, has performed a difficult task with the masterly skill and tact of an experienced literary craftsman. The record embraces the boyhood and youth of McCosh, his university career at Glasgow and Edinburgh, his experience as a minister during which he played his part in the memorable disruption and the establishment of the Free Kirk of Scotland, his career as a professor at Belfast and a leader in the national education of the Scotch and Irish, closing with the splendid chapter which his twenty years at Princeton added to the educational history of that university and the country. The whole story gives a strong impression of the simplicity as well as the greatness of the man and will enable the public to understand the secret of his immense influence at Princeton and the profound impression which he was able to make on the educators of his generation. Space will permit only an allusion to the educational services of Dr. McCosh and we must hasten to notice his work as a thinker and philosopher. Some of the most important of his services in this line have been rendered as a leader in a movement of transition and adjustment. Such, for example, was his attitude toward evolution which, as a religious thinker, he adopted and defended as an ally rather than a foe

to religion, at a time when evolution was generally regarded as atheistical. Such also was the service he rendered the new physiological psychology at a time when traditional methods were almost universally prevalent. Although not distinctly experimental, Dr. McCosh's method was largely observational and his works are treasuries of facts and shrewd observations. In philosophy Dr. McCosh stands in line with the best Scottish traditions. He was a stout champion of a realistic epistemology and an intuitionist metaphysics. His real contribution to philosophy consists, however, not so much in any special doctrines which he may have taught as in certain fundamental convictions, metaphysical, ethical and religious, which inspired all his work. In his advocacy of these he was able to exert a profound influence upon his age and, at the same time, to make an important contribution to its thought.

PRINCETON.

A. T. ORMOND.

Contemporary Theology and Theism. R. M. WENLEY. New York, Charles Scribner's Sons, 1897. Pp. 197. \$1.25.

We congratulate Professor Wenley and the public upon the happy thought which prompted him to mark his advent to an American university by the publication of this little book, part of the material for which was originally presented before the Theological Society of Glasgow University in the form of an address. Professor Wenley is favorably known as the author of *Socrates and Christ* and *Aspects of Pessimism*, and as a contributor to the philosophical journals. The present volume, like the earlier ones, is, in the main, critical and expository rather than constructive, but the constructive element is sufficient to define the author's position among contemporary students of the philosophy of religion. In the brief space here available one can do little more than cordially commend this essay to all who are interested in contemporary Theology and Theism—to the lay reader as well as to the professed student of these subjects.

The author's purpose may, perhaps, fairly be said to embrace a threefold aim, viz., to show the influence of philosophical theory upon current theological thought, to offer some criticism of the theology resulting from an inadequate philosophy *overriding* facts and warping their interpretation, and finally to ask whether theology can not in its turn add something to philosophy, and so contribute toward the formation of a more adequate philosophy of religion. The first half of the volume furnishes cogent illustration of the historical, as well as of the logical, inseparability of philosophy and theology—a fact which

should not, but which does, need ever fresh iteration—and offers some acute and valuable criticism of the two main currents of contemporary theological thought which have been chiefly determined by their respective philosophical presuppositions. The speculative school is that which, building ultimately on Hegel, construes the historic facts of religion, and of the Christian religion in particular, in accordance with the logical necessity of the Hegelian dialectic; which the Ritschlian school, building on Kant and Lotze, so separate philosophy from religion, dogma from fact, that it holds a Christianity divorced alike from metaphysics and from history, and resting on no objective basis of fact. These two schools do not of course adequately represent contemporary theology, since there are also the 'mediating' theologians and the conservative school to be noted. In this regard, therefore, Professor Wenley's title is bigger than his book, and to this extent it is misleading. But his exposition of the two theological tendencies with which he deals is clear and fair; his criticism of their defects is acute, and the reader who is not particularly acquainted with the movements of recent theology will doubtless retain a more vivid impression from this bird's-eye view of two of its phases than he would from a more expansive and detailed presentation.

The latter half of the volume deals with 'the theistic problem.' The question is, "Can theology, accepting the metaphysical first principles which spiritual inquiry of necessity involves, so react upon philosophy as to produce a less inadequate solution of difficulties?" Professor Wenley answers, yes. There are at least three regions where theology can assist and correct philosophical speculation. These are "the questions of the personality of God, of the creative or originating power which marks the divinity of Christ, and of the relation of man to sin." It is with the first of these three, or with the theistic problem proper, that the remainder of the book is chiefly concerned—at first by way of criticism of the agnostic and gnostic positions respectively, and then in offering some constructive suggestions toward the solution of the problem. Possibly the most important sections of this portion of the book are those which contain the very discriminating and appreciative estimate of Hegel, and the suggestion that the key to the solution of the theistic problems may be found to lie in a more perfect analysis of the idea of personality. The *Hauptproblem* is how 'to preserve the requisite balance between immanence and transcendence.' The author thinks he finds the clue to the resolution of this difficulty in the finite self, which combines the qualities of immanence and transcendence, and so furnishes an analogy for the

nature of God. The hint here let fall seems to us full of suggestiveness, but Professor Wenley has not worked it out sufficiently to make his meaning altogether plain. We close with an expression of the hope that he may yet be able to do this for us in the more systematic and constructive work of which we trust the present essay is the precursor. Meanwhile it may perhaps be worth noting that his general point of view is not unlike that of Professor Fraser in that he too starts from man and man's experience as the clue to the nature of divine personality.

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PEDAGOGICAL.

Ueber eine neue Methode zur Prüfung geistiger Fähigkeiten und ihre Anwendung bei Schulkindern. H. EBBINGHAUS. Ztschr. für Psychol. u. Phys. der Sinnesorgane, XIII., 401-459. 1897.

In 1895 the city authorities of Breslau applied to the Hygienic Section *der Schlesischen Gesellschaft für vaterländische Kultur* for an opinion and report on the advisability of holding school sessions five hours long. The secretaries of the Section, Professors Flügge, H. Cohn and Jacobi, added to their number several other physicians and educators, including Professor Ebbinghaus, who has given us the above account of the preliminary labors of the commission.

The method of Burgerstein with addition and multiplication of simple numbers, that of Sikorski and Höpfner with long dictation exercises, and that of Richter with easy algebra and Greek conjugations, were all objectionable since they did not preserve sufficiently the normal character of a recitation period. They aimed to measure fatigue but vitiated the results by the monotony and lack of interest due to their methods.

A second set of investigators, recognizing this, have avoided interfering with the normal school-work, but apply an appropriate test from time to time, to determine the amount of fatigue due to the regular work. Thus Griesbach tested pupils at different periods in the course of the day by measuring their sensibility to touch, and found that it varied with their mental fatigue.

The Breslau commission determined to combine the best features of both methods; they allowed the ordinary school work to take its regular course, but tested the pupils before school and at the end of every period by having them spend 10 minutes in (1) adding or multiplying

(arithmetic test), or five minutes in (2) writing numbers of from 6 to 10 places from dictations (memory test), or (3) filling in omitted syllables and words in a specially prepared text (combination test). The tests were made in a gymnasium and in a girls' school, on three different Wednesdays a fortnight apart.

The second test, that of the memory span for numbers, showed most remarkable variations from period to period and seemed least reliable when the tests were not all made by the same teacher so as to insure uniformity in rate, rhythm, tone, etc., in giving out the numbers.

The third test was intended to go deeper and test intellectual fatigue. The omitted syllables were indicated by dashes and pupils were required to restore the omissions as rapidly as possible, but always *so as to make sense*. In general this test brought out greater differences in the several classes than either of the other two methods. By this method of testing Untertertia accomplished more than twice as much as Sexta and made an average of less than one-third as many mistakes, whereas by the arithmetic test the difference was less than 25% increase in these three years.

The three methods showed interesting differences within each class as well as from class to class. For this purpose each class was divided into three groups according to their ranking in scholarship. The memory test showed quite as good results, or even better among the duller pupils than among the brighter ones. The arithmetic test placed the duller pupils midway between the brighter and the mediocre ones. The combination test, however, reflected with great fidelity the rank and scholarship of the pupils. The quantity of work as well as the quality of it increased regularly in every class from the duller to the brighter pupils. The differences between the three groups were much greater in the lower classes and least in the highest classes.

In the lower classes the girls were without exception behind the boys in all three tests, but in the higher classes the sixteen year old girls had completely overtaken the boys of corresponding age.

The memory test showed no sure signs of fatigue at the end of five hours of school work. The arithmetic test brought out evident weakening in effectiveness and accuracy, while the combination method gave no sure signs of fatigue in the upper and middle classes at all. Pupils of 10 to 12, however, undoubtedly fatigued much more rapidly. Whether this fatigue is *harmful* or *useful* is not shown by these tests and would require other tests to determine the fact. It is to be hoped the Commission will carry out these further investigations, for it is certainly a very effeminate pedagogy that is going to try to keep the dear children from ever getting tired.

Lastly, the results were worked over to compare the effects of different branches of study. After language lessons in the classics the combination test showed considerably better results both in quantity and quality, than after lessons in any other branch, *e. g.*, science, arithmetic or drawing; notwithstanding that these subjects afforded less mental strain of attention and consequently probably less fatigue.

The Pedagogical Seminary. Edited by G. STANLEY HALL. Vol. IV., 2 and 3. December, 1896, and April, 1897.

'A Study of Dolls,' by Mr. Ellis and Dr. Hall gives the substance of an extensive and laborious collection of data, tabulation of statistics and rare suggestions of applications. The chief topics are: material of which dolls are made, substitutes and proxies, psychic qualities, doll's food and feeding, sleep, sickness, death, funeral and burial of dolls, doll's names, discipline, hygiene and toilet, doll's families, schools, parties, weddings, accessories and furnishings, miscellaneous anthropological notes.

The doll passion seems to be strongest between seven and ten and reaches its climax between eight and nine, and the parental instinct is far less prominent in doll play than is commonly supposed. However disconnected the words doll and idol, some psychic connection cannot be doubted. Idols may, perhaps, be valuable object lessons in religion for children at the pagan stage and may yet have a rôle to play in elementary religious training. The small scale of the doll world focuses and intensifies affections and all other feelings.

Although doll play educates the heart and will even more than the intellect, many school subjects are also helped by it. Children with French dolls incline to practice their little French upon them; can this tendency be utilized in teaching a foreign language to young children? Some children thus learn to read, sew, knit, do millinery work, observe and design costumes, acquire taste in color and even prepare food, they make their dolls represent heroes in history or fiction and take them on imaginary journeys into foreign lands, and sometimes the doll serves as an ethical ideal and helps them to be good. Dolls are an excellent school for children to practice all they know of rudimentary sociology, ethics and science. Would not dolls and their furnishings be among the best things to make in manual training schools? Why are dolls, which represent the most original, free and spontaneous expression of the play instinct, so commonly excluded from kindergartens, where they could aid in teaching almost everything?

"There should be somewhere (a) a doll museum, (b) a doll expert to keep the possibilities of this great educative instinct steadily in view, and (c) careful observations upon children of kindergarten, primary and grammar grades should be instituted as at an experiment station in order to determine just what is practicable."

Mr. Small's study of the 'Suggestibility of Children' presents a great deal of concrete material, partly experimental and partly observation notes in answer to a syllabus. He concludes that in healthy children a high degree of suggestibility is a universal condition and largely within the control of any one in sympathy with children. Hence the necessity of removing from the public schools, stutterers, emotional prodigals, and nervous defectives; greater prominence of motor element and dramatic instinct in learning; a possible use of the social instinct as it crops out in school fads to awaken interest in history, literature and science; a hint at the natural method of child discipline in suggestion as children use it; and the strong influence of the attitude of the teacher upon the tastes and ideals of the pupils.

Mr. Dawson's 'Study in Youthful Degeneracy' gives us the results of a difficult and embarrassing study of sixty juvenile delinquents, comprising carefully selected types of (1) thieves, (2) incendiaries, (3) assaulters, (4) sexual offenders, and (5) general incorrigibles.

In the April number Mr. Street reviews the chief methods of language teaching and Mr. Crowell summarizes the 'Courses of Study in the Elementary Schools.' Mr. Burk has worked over a great many returns to a questionnaire on 'Teasing and Bullying' and believes that 'these are to be classed more as crystallized instincts than as conscious and voluntary activities.' He suggests that the movements involved are 'the racial form of all exercise,' and that as such "they are the only possible forms of exercise upon which progress in physical development, and mental development, of the individual rests."

Mr. Partridge has contributed two short articles on 'Second Breath' and 'Blushing,' and Miss Frear, of Stanford University, has worked out in a series of six charts a number of general conclusions based on the material in Mr. Russell's book on imitation.

The work in these two numbers of the *Seminary* is based almost entirely on the returns to President Hall's Child Study Syllabi, and the authors have taken advantage of this rich concrete material for copious use in illustrating all the points brought forward. Notwithstanding the able and thoroughly practical conclusions of most of the papers, the chief inspiration of it all lies in the plain, unvarnished ob-

servation notes that formed the raw material for these studies and might form the basis of dozens of still other 'conclusions.' The advantage of publishing the original material is obvious in affording opportunity for further interpretations.

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VISION.

Ueber intermittirende Netzhautreizung. FR. SCHENCK. Pflüger's Archiv, Bd. LXIV., 165-179, 607-628.

On Intermittent Stimulation of the Retina. Part I. By O. F. F. GRÜNBAUM. Journal of Physiology, XXI., 396-403.

An Account of Certain Phenomena of Colour Vision with Intermittent Light. G. J. BURCH. Journal of Physiology, XXI., 426-434.

Much interest has been aroused by the method of photometry introduced by Professor Rood in 1893 (Am. Jour. Sci., XLVI.), which is based upon the fact, first observed by Plateau, that there is a definite relation between the intensity of two alternating light-sensations and the rate of frequency of repetition necessary to cause them to become fused, that is, to cause 'flicker' to become extinguished. The less the difference of intensity of the two excitations, the less rapidly do they need to alternate in order to produce a homogeneous intermediate sensation; if a disc is half white and half black, it must rotate more rapidly to extinguish flicker than if it is half a light gray and half a dark gray, and the less the difference in the grays the less is the rapidity of rotation that is required. This circumstance gives an evident foundation for a method of photometry, which is of particular advantage for the estimation of the brightness of different colors, since the color constituent is found by most people to be very disturbing in estimating relative brightness by plain inspection; by this method it is only necessary to select from a number of grays of known brightness the one with which the color in question will most readily fuse.

Schafhäütl (Münch. Akad. Abh., VII.) had already proposed in 1855 a photometric method based upon the extinction of flicker, which should give absolute intensities and not simply comparative ones; he looked at a bright surface through a hole behind which a small screen was caused to vibrate which alternately shut out and let through the light from the surface to be examined. He assumed that the rapidity

of vibration of the spring would be proportional to the square root of its length (which would not be the case when the spring carries a weight) and that the intensity of the light when flicker just ceased would be proportional to the square root of the rate of vibration of the spring (which is also not known to be true).

Schenck proposes, in his second communication, a modification of the method of Rood, by which the color to be tested is placed upon a color-disc on which there is a gray which goes gradually, from the center outwards, from white to black; this is secured by painting black upon a white surface in such a way that the amount of black at any given narrow ring of the disc is proportional to the distance of that ring from the center. He then looks at the rotating disc through a small hole in a piece of cardboard, and determines at what distance from the center fusion takes place with the lowest possible rapidity of rotation; this will be the position of that black and white mixture which is of equal brightness with the color which is being tested, and the proportion of black and white in it will be given by its distance from the center. Outside and inside of this ring, flickering is still going on, because the gray is either too dark or too light to fuse with the color at so low an intermittence frequency. The method was found to work well. It was tested by determining the brightness of each of two complementary colors, and then the brightness of their resulting gray light and comparing this last with the brightness computed for the two colors when mixed in the proportion necessary to give gray; the coincidence was very close. This method of testing was of course made use of by Rood, and described by him in his first communication. But the curious circumstance developed itself that when the brightness of the papers was determined by direct inspection—by choosing the gray which seemed to look equally bright with a given color—very different results were obtained. The two brightest colors, yellow and green, were given as much too bright by the intermittence method, yellow especially so, while all the other colors, and particularly red, were given as too dull. No explanation has been found by Schenck for this discrepancy. The idea of Hering that complementary colors have an opposite and compensatory specific brightness effect does not apply, because here yellow and green belong in one category, and red and blue in the other. Moreover, there is no extinction of the color in this experiment, it is merely spread in a thinner layer over a larger retinal surface; therefore, there would be no sense in assuming that the intermittence method determines the white-valence alone, and by the test already referred to, it is evident that there

is exactly determined by the intermittence method that element of brightness (whatever it may be) which goes to the formation of the brightness of the gray produced by the mixture of two complementary colors; from which it results that Hering's idea of the specific brightening and darkening power of the four colors is as meaningless and confusing when it comes to a practical application as it is in theory. The mere inability to detect by direct inspection the relative brightness of two different colors seems to be also no sufficient explanation, because it would appear that *some* definite affection of sensation is got by this means which is common to different observers and to the same observer at different times. The subject would apparently repay further investigation.

A curious circumstance was first noticed by Filehne, in 1885, in connection with the fusion into one mean sensation of two rapidly alternating sensations. If two discs are prepared, one of four alternate equal black and white sectors and the other of sixteen, and if the first be given a rotation velocity four times as great as that of the second, then the rate of alternation of black and white excitations upon a given point of the retina is alike in both cases, but, nevertheless, the conditions are not equally favorable for fusion; the rapidly rotating disc will present a fusion of sensation at a time when flicker is still persisting in this disc of many sectors. Mere linear velocity seems in some curious way to assist the fusion. Thirty alternations per second suffice to produce fusion in the one case, while if the sectors are numerous and the disc rotates in the same proportion slower, flicker may persist with over seventy alternations per second. Fick found that when parallel lines were drawn on a drum which rotated about an axis parallel to the lines, as many as 170 alternations a second might be necessary to produce fusion, but that if the moving lines were looked at through a slit, flicker ceased at forty per second. He suggested that this discrepancy was owing to the fact that when the speed of translation is slow the eye more readily follows the moving contour of the sectors, and the alternating excitations do not fall in order upon exactly the same part of the retina, but that the use of a small aperture for observation prevents this movement of the eyes. Schenck's first paper is devoted to upholding this view as against Marbe, who maintained that the slow contour movement in itself is enough to retard fusion. He does this first by experiment, and he then shows with much skill that the theoretical considerations by which Marbe has sought to deduce his view as to the effect of contour motion from his theory of Talbot's law are ineffective, and also that his theory is at

bottom not different from the usual theory, and especially not so well stated as by Boas (Ann. d. Phys. u. Chem., N. F. XVI.). To sum up, the moments which affect fusion favorably are these:

1. Diminution of the duration of the double period.
2. Increase in the difference of duration of the two separate excitations.
3. Diminution in the difference of intensity of the two excitations.
4. Increase of the absolute mean intensity.
5. More rapid contour motion (in the case of rotating discs).

Marbe's explanation of the effect of the first four of these circumstances is the same as that of Boas. His explanation of the last, which is that it is due to contrast, is counter-indicated by an experiment of Baader's, in which a disc is prepared of alternate black and white half rings, and it is found that fusion takes place just as well as with solid half circles of black and white, in spite of the fact that adjoining rings upon the retinal surface are in the first case constantly in opposite phases of excitation, and hence favorable to the production of contrast. Schenck himself seems to think that, when fusion is prevented by reason of eye-movements, it is by means of a psychical effect, *ein deutliches Erkennen der Conturen*; does he not here overlook the very evident fact that when the eye follows the contour a given part of the retina is exposed for a longer time to white and respectively to black and that there is, therefore, a physical effect which is exactly the same as if the disc were rotating more slowly?

Mr. Grünbaum's paper presents a degree of obscurity in the description of a sufficiently simple experiment which one would have to go far to see equalled; in grammar even it is not above reproach. His experiments show apparently that even the use of an aperture does not do away with what we may call the Filehne anomaly, described just above, *unless* there is a constant relation between the size of the aperture and the cross-section of the black and white disc-sectors which are sweeping past it. Consider for a moment what would be the effect of a non-constant relation between the two quantities just named: let *a* and *b* be two equal discs, each with alternate equal black and white sectors, but let the individual sectors of *b* be ten times as large as those of *a*, and at the same time let them rotate ten times as rapidly. If they are looked at through apertures at the same distance from the center of each disc, then the conditions as regards any given retinal point will be alike in both cases, it will be subjected to alternate black and white excitation in periods of the same duration. But there will be a difference as regards the square surface of the

retina as a whole upon which the image of the aperture falls. If the black and white sectors are no wider across at the point examined than is the aperture, then there will be no perceptible time during which *the whole aperture* is black or is white, but if the sectors are the large and the rapidly moving ones, then the whole aperture will be a good part of the time exposed wholly to either black or white. The former case, according to Grünbaum, is favorable to simultaneous contrast, and hence the difference in physiological intensity of the two stimuli is increased, and fusion is interfered with. He refers to Sherrington's paper (about to be noticed) for proof of this effect of contrast; but Sherrington found, under favorable circumstances, that contrast caused 34 rotations per second to be essential to fusion when without it 22 were sufficient, while Grünbaum gets, for changing breadth of sector (everything else remaining the same) a change of number of rotations from 43 to 225. It is difficult to believe that such a difference as this could be due to contrast. Moreover, are not the conditions as favorable to successive contrast in the latter case as they are to simultaneous contrast in the first? Grünbaum considers it improbable that 'when an aperture of 5 mm. is used and the eye focussed for a cross drawn upon the screen' (by which he doubtless means to say that the center of a cross is fixated) any movement of the eye can occur. One might equally well say that, under these circumstances, very small movements of the eye, which are known to be unavoidable, would be sufficient. He neglects to say that his explanation is the same as that given by Marbe, and criticised as above by Schenck.

Burch experimented with spectral light, which he made intermittent by means of a rotating screen pierced with holes. His double period consisted of a short duration of very bright light and a long duration of darkness. When the dispersion was wide, so that the field of view of the spectroscope was sensibly of one color, and when the rotation was too slow to produce fusion, he detected patches of darkness of the same shape as the interstices between Purkinje's figures. With a very short duration of the flash, the yellow spot of the retina became subjectively evident; under certain circumstances "upon looking steadily at the part inside the bend of the absorption band between *C* and *D*, it is seen to be occupied by an irregular group of brilliant red dots on a ground of beetle-green or steel-blue." When the flashes were of very great intensity, instead of a continuous spectrum there were seen three bands of color, red, green and blue, upon a brightly illuminated whitish background. The explanation given of this latter phenomenon by Mr. Burch is very ingenious, and, as it happens, it fits in very well

with my theory, in fact, it is much the same as the explanation that I have given, under simpler circumstances, for the lesser purity of the portions of the spectrum between the fundamental colors in general. It is this: a given color-decomposition (if we speak in chemical terms, for the sake of clearness), will be effected chiefly by a certain oscillation frequency of light, which we may call its optimum period, but it will also be effected to a less extent by other rates on either side of this. Now when the light employed is very intense, a maximum decomposition will be effected by periods at some distance on either side of the optimum period. With a steady illumination, this would make the whole spectrum whitish, and very bright, but with an intermittent illumination, the resulting sensation is not so intense as to prevent the observer from recognizing the greater apparent brilliancy of those portions where two color-sensations overlap, and accordingly those parts look brighter than the rest and have the pale tints of binary color-blends. The author apologizes for this explanation on account of the fact that it posits red, green and blue (in opposition to Hering) as the primary colors. (He saw violet, under certain circumstances, as very bright also, but an easy explanation lies at hand for this—there are only a few red-producing rays at that end of the spectrum with which to diminish the purity of the blue.) This is, however, an element in its favor, and he has moreover other observations, not yet published, which will confirm this view.

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BALTIMORE.

Neue Versuche über intermittende Gesichtsstreize. KARL MARBE.
Phil. Studien, XIII., 1. 106-115.

The author investigates the relations between the critical period of duration of intermittent visual stimuli and the average brightness (Helligkeit) of the stimuli. "For two visual stimuli which fall upon the retina successively and periodically, there is a certain short period of duration in which they produce a constant sensation." This the author calls the critical period of duration. According to Baader, the critical period grows for two colorless stimuli, as the difference of brightness between the two sensations decreases. This is true alike when the average brightness increases with the increasing difference between the stimuli, and when the average brightness is constant. Kleiner showed that, with a difference of stimuli increasing from 0 on, the critical period decreases at first very rapidly, then slower and slower, until finally the decrease almost ceases. The author asks and

answers the questions whether the conclusion of Kleiner holds for all cases of increasing stimuli, indifferent whether the average brightness of the stimuli increases, is constant, or decreases; and whether, if this is the case, the regularity is determined by the differences in the stimuli.

Author used 40 gray pieces of paper of different degrees of brightness determined photographically, as described in the *Zeitsch. f. Psych. u. Physiol. d. Sinnesorgane*, Bd. XII., S. 62f. The brightest piece reflected about 13 times as much light as the darkest, the determinations being made by the Kirschmann photometer. Degrees of brightness between white and black were determined as follows: A white and a black disk of 16cm. diameter were placed on a Maxwell color-mixer. Concentrically over these was placed the gray disk whose intensity was to be determined. Then the white and black disks were adjusted to give the same brightness as the gray disk, starting first from a mixture which was clearly brighter, then from one which was clearly darker than the gray disk, and taking the average of the two determinations, which is given in the tables. The method by which the rapidity of rotation was determined which is necessary to give a constant sensation from the two stimuli, is described in *Phil. Studien*, Bd. IX., S. 389ff. Driving force was produced by an electromotor with an Ad. Fick regulator.¹

The author's conclusions are as follows: (1) With increasing difference between two stimuli the critical period of duration decreases at first very rapidly, then more slowly, and finally almost none at all. (2) This holds indifferently, whether the average intensity increases or decreases with increasing difference of stimuli. (3) The values of the critical periods of duration are determined, for the most part (*im wesentlichen*), by the objective, not by the subjective, differences between the two stimuli. (4) To equal objective differences correspond about equal critical periods of duration.

The article includes five tables and three curves. The account of the experiments is somewhat condensed and brief, but probably a more detailed account is unnecessary. The subjects were the author and Professor Külpe.

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¹Described by Bradt: *Ueber die Wärmebildung bei summirten Zuckungen des Muskels*. Wurzburg, Etlinger's Buchdruckerei. 1893. S. 13ff.

VOLITION.

The Psychology of Effort. JOHN DEWEY. Philosophical Review, VI., 43-56, January, 1897.

Professor Dewey here presents a theory of the psychology of effort in harmony with his theory of the significance of emotion (this REVIEW, II., 13 ff). Accepting the sensationalist view of the consciousness of effort, he finds the specific quality of this consciousness in the rivalry between the sensation of motor adjustment and the sensori-motor idea of the desired end, with the accompanying disagreeableness due to failure of habit. The scandal of the assertion that awareness of effort is a sense of changes of breathing, of muscular tensions, etc., is removed, he thinks, when it is explained that "these sensations report the state of things as regards effective realization." The theory explains the increase of the sense of effort in fatigue psychologically—it is due to the introduction of new distracting elements; other theories fall back on the exhaustion itself, an extra-psychical factor. It also explains certain facts in connection with the mastery of novel acts; in learning to ride a bicycle, for example, if the more habitual motor adjustments fail to get transformed so as to correspond with the image of the desired balancing, the sense of effort may be at a maximum, but if the movements become utterly unregulated, so that the consciousness of the end aimed at disappears, then, notwithstanding the mass of muscular sensations, the sense of effort vanishes also. Dewey denies that the sense of effort arises from an activity struggling against resistance. The appearance of such a struggle he explains as due to the importance attached to the motor adjustment as means. If this fail, then all lying outside it is regarded as resistance. "The real state of things is that there are two acts mutually opposing each other during their transformation into a third new and inclusive act." He also opposes the view that it arises from the self endeavoring to overcome obstacles. The whole process is one of divided self-activity, not that of an active 'self' on the one side as over against muscular resistance on the other.

As in the theory of emotion, the 'scandal' of the sensationalist view appears to the present writer to lie not so much in the assertion that the sense of effort is the feeling of bodily sensations as in the isolation of these sensations and the appearance of treating them as though they existed in the experience itself in the same form in which they exist for our psychological abstraction. Admit them as in actual experience elements in a specifically related mass of conscious contents,

admit them as the feeling, the sense-awareness of a struggle of adjustment in which the actual self of the moment, self-divided, is seeking expression in a complete action, and who is there that cannot subscribe to the theory?

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Über willkürliche Vorstellungsverbindung. STEPHAN WITASEK.
Zeitschrift für Psychol. u. Physiol. d. Sinnesorgane, XII., 3 und 4, Oct., 1893.

A difficult and interesting topic is handled by Witasek after a somewhat inadequate and an unduly diffuse fashion. His subject is the nature of *willkürliche Vorstellungsverbindungen*, that is, of volitions which have as their 'objects' psychic facts, not bodily motions. How, for instance, can one be said to 'will' to imagine a three-fourth rhythm, or the 'color designated by the Fraunhofer line B?' Anticipatory image of color and of rhythm there must be, or there is no volition, yet the anticipatory image can not be precisely like the intended one, else the supposed volition will coincide with its object. Witasek answers by distinguishing the anticipatory image as un-perceptual (*unanschaulich*), from the concrete image which is the result of volition, while he observes that they are alike in referring to the 'same thing'; since, however, such a sequence of un-concrete upon concrete may be an affair of purely involuntary association, he emphasizes the additional consciousness of the relation between the two. He proceeds to analyze the solution into four psychic factors: (1) the act of will (*Willensakt*), (2) the unperceptual anticipatory image of the object, (3) the relation between the anticipatory image and (4) the concrete image which is the object of the act of will.

Witasek's exposition of this analysis discloses its weak features. There is in the first place, no justification whatever of its first moment, the 'act of will' which proves to be a perfect fifth wheel to the coach (see p. 211). The 'relation' between (2) and (3) is another contraband article in modern psychological writing; it might better be treated after Dr. James' fashion as a 'transitive element' of the anticipatory image itself. In fact, the greatest value of the discussion is its recognition of the problem of inner volition, its emphasis upon the difficulty of the distinction between volition and object of the volition, when the latter is itself a fact of consciousness. The real nature of the distinction, however, is only suggested by the description *unanschaulich*, which, if one may judge from the illustrations offered,

virtually means 'verbal.' On the other hand, the different emphasis of attention in the case of anticipatory and of resultant image is not adequately considered, for (in the opinion of the writer of this notice) attention is the x in terms of which the problem must be solved.

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EMOTION.

The Sense of Beauty, being the Outlines of Æsthetic Theory

GEORGE SANTAYANA. New York, Charles Scribner's Sons.
1896. Pp. ix+275.

Perhaps the first thing to be mentioned about this book is its perfection—if the word be not too cruelly pressed—its flawlessness. It is an unpadding little masterpiece—it fills its covers as an athlete fills his skin, it 'pays' its way sentence by sentence down the page. It makes 'no pretensions to originality beyond that of putting together the scattered commonplaces of criticism, under the inspiration of a naturalistic psychology'; but the inspiration has been sincere, and the commonplaces have been not only reset, but recut, and the 'cutting' is often, in its unobtrusive way, exquisite. Granted its point of view, it is all thought out with an extraordinary quietness and completeness and unisistent finish; and the artistic imagination has everywhere been discreetly busy with its phrase.

It 'contains the chief ideas gathered together for a course of lectures on the theory and history of æsthetics given at Harvard College from 1892 to 1895.' It consists of a brief 'Preface,' from which the foregoing sentence is quoted; of an 'Introduction,' on the 'Methods of Æsthetics'; of four 'Parts,' on the 'Nature of Beauty,' the 'Materials of Beauty,' 'Form,' and 'Expression' respectively; and of a concluding chapter. There is also an analytical table of contents and an index. The 'Method' recommended (it has been indicated already), is the psychological, as distinguished from the historical and from the didactic. Æsthetics is the theory of a certain kind of 'values,' and values are subjective. "We desire nothing because it is good, but it is good only because we desire it." "Things are interesting because we care about them, and important because we need them. Had our perceptions no connections with our pleasures, we should soon close our eyes on this world; if our intelligence were of no service to our passions, we should come to doubt in the lazy freedom of reverie, whether two and two make four."

The problem of the 'Nature of Beauty,' therefore, is simply to distinguish the æsthetic pleasures from the non-æsthetic. And this distinction does not lie in the supposed 'unselfishness' of æsthetic pleasures. Selfishness and unselfishness are not of the essence of any pleasures whatever, they are accidental, extrinsic. "There is no reference to the nominal essence called oneself in one's appetites or in one's natural affections; yet a man absorbed in his meat and drink, in his houses and lands, in his children and dogs, is called selfish because these interests, although natural and instinctive in him, are not shared by others. * * * I care about myself because *myself* is a name for the things I have at heart. To set up the verbal figment of personality and make it an object of concern apart from the interests which were its content and substance, turns the moralist into a pedant and ethics into a superstition."

Neither does it lie in the supposed universality of æsthetic pleasures. "The pleasures of the senses have, it is said, no dogmatism in them; that anything gives me pleasure involves no assertion about its capacity to give pleasure to another. But when I judge a thing to be beautiful, my judgment means that the thing is beautiful in itself, or (what is the same thing more critically expressed) that it should seem so to everybody." But preference of every sort is ultimately irrational and it is simply unmeaning to say that what is beautiful to one man *ought* to be beautiful to another. If their senses are the same, their associations and dispositions similar, then the same thing will certainly be beautiful to both. If their natures are different, the form which to one will be entrancing will be to another even invisible, because his classifications and discriminations in perception will be different, and he may see a hideous detached fragment or a shapeless aggregate of things in what to another is a perfect whole. It is absurd to say that what is invisible to a given being ought to seem beautiful to him. Evidently this obligation of recognizing the same qualities is conditioned by the possession of the same faculties. But no two men have exactly the same faculties, nor can things have for any two exactly the same values.

The distinction lies, paradoxically enough, in the accomplished objectivity of æsthetic pleasures. "Every sensation we get from a thing is originally treated as one of its qualities. The qualities which we now conceive to belong to real objects are, for the most part, images of sight and touch. * * * But emotions are essentially capable of objectification, as well as impressions of sense; one may well believe that a primitive and inexperienced consciousness would rather people the

world with ghosts of its own terrors and passions than with projections of those luminous and mathematical concepts which, as yet, it could hardly have formed."

In process of time, however, such concepts are formed, and the list of pleasures objectified is retrenched—mainly on the ground of their association with some particular organ of the body, like the palate. "The pleasures we call physical, and regard as low, * * * are those which call our attention to some part of our own body, and which make no object as conspicuous to us as the organ in which they arise." The residue, the pleasures that are unreclaimed, those whose 'organs' are transparent, are the æsthetic. "The scientific idea of a thing is a great abstraction from the mass of perceptions and reactions which that thing produces; the æsthetic idea is less abstract, since it retains the emotional reaction, the pleasure of the perception as an integral part of the conceived thing."

Beauty, therefore, is 'pleasure regarded as the quality of a thing.' The 'Materials of Beauty' are to be found in the various susceptibility of the human frame to pleasure, in especial, among others, to the pleasure that unites the sexes. "The capacity to love gives our contemplation that glow without which it might often fail to manifest beauty; and the whole sentimental side of our æsthetic sensibility, without which it would be perceptive and mathematical rather than æsthetic, is due to our sexual organization remotely stirred." For individuals that "need not unite for the birth and rearing of each generation, * * * it would not be necessary that any vision should fascinate, or any langour should soften, the prying cruelty of the eye. * * * Sex is not the only object of sexual passion. When love lacks its specific object, when it does not yet understand itself, or has been sacrificed to some other interest, we see the stifled fire bursting out in various directions. One is religious devotion, another is zealous philanthropy, * * * but not the least fortunate is the love of nature and of art; for nature is also often a second mistress that consoles us for the loss of the first." Beauty of 'form' is essentially bound up with the intrinsic agreeableness of certain kind of muscular tension, and beauty of 'expression' is a special case, simply, of psychological suggestion.

This is the main thread of the argument, but the pages abound in the discussion of minuter points and in that *exercice méthodique du discernement* which has been declared to be the essence of criticism.

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BRYN MAWR.

Ästhetische Untersuchungen in Anschluss an die Lipps'sche Theorie des Komischen. I. and II. G. HEYMANS. *Zeitschrift für Psychologie u. Physiologie der Sinnesorgane.* XI., 1 and 5-6, April and July, 1896.

Heymans finds in the Lipps theory of the comic¹ what he calls the 'final and definitive solution of the old problem,' but nevertheless discovers certain inadequate features on which he comments in his first paper. Lipps holds that the consciousness of the humorous is roused when a high degree of psychic force is lavished upon a trivial or unessential content of consciousness, and with this statement Heymans is in full agreement; but he denies the universality of the second form in which Lipps states his theory, the assertion that the humorous object of consciousness is always a meaningless one following upon one which is significant. Many cases of the comic, of course, fall within this class, but the real contrast involved is between a content attended-to—that is, in the Herbartian terminology of Lipps and Heymans, a content requiring an expenditure of 'psychic force'—and another which makes no such demands upon the attention. Therefore, the earlier object need not be in itself significant, but may be attended to merely because it is unexpected. Heymans illustrates by misprints, which are never funny when the incorrect words are wholly meaningless, but only when they appear to be *bona fide* words, so that the contrast is between the surprised attention to a word, however unimportant, at variance with the context and the sudden *intuition* of the word intended which needs no special emphasis.

Heymans also instances cases to show that Lipps is mistaken in requiring that the contrast occur between contents which are qualitatively alike. The paper is least effective in the explanation of laughter following on sudden relief from deep-seated feelings and impulses, for here Heymans yields to the temptation of making laughter a certain indication of the feeling of the comic, whereas it is surely often a mere physical reflex, and, at other times, an accompaniment of surprise untinged with the comic consciousness.

In his second paper Heymans develops a suggestion of Lipps into the theory that the beautiful is the object of facile attention. The object of the æsthetic consciousness thus calls forth the same psychic energy as the preceding content of consciousness, and is distinguished from the comic object, which demands less psychic force, and from the terrifying object, which calls for more. Heymans attempts

¹ *Psychologie der Komik.* Theodor Lipps, *Philosophische Monatshefte*, XXIV & XXV.

to prove his case by an analysis of traditional classifications of the beautiful, discovering that the 'formally beautiful,' by its unity in manifoldness, and the 'typically beautiful' by its conformity to the habitual, do really facilitate attention. Two other classes of the beautiful are considered; the 'imitative,' which, however, at once reduces to one of the other classes, or else turns out to be no form of the beautiful at all, and the 'associatively beautiful.' Heymans correctly defines the associated element of the æsthetic object as that which itself has a tendency to associate, and thus to emphasize, the perceived part of the object, but he seems not to realize that by this analysis he really opposes the association-theory of æsthetics, since he admits that a percept is beautiful, not because it is associative, but because it absorbs attention so completely that associated elements, if they occur, are unattended to. The comparison of the 'interesting' machine, with the 'beautiful' landscape, clearly shows that the presence of associated factors—images of utility and result—which draw the attention from the object itself, hinders æsthetic apprehension.

A closer examination than Heymans gives would prove that one main characteristic of the 'beautiful' objects is its isolation, its unrelatedness, its entire separation from any considerations of utility or any definite reference to past or future. But Heymans admits enough of this to endanger his entire theory, since he really shows that not every object of attention, but only the perceived or imaged object of direct attention, is beautiful; attention is indeed then an important characteristic, but not as he teaches, the essential feature of the æsthetic consciousness.

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La Timidité, Étude Psychologique. L. DUGAS. *Revue Phil.*, December, 1896.

This article is chiefly an analysis. The term timidity is used broadly to designate the emotion caused by inhibition of action, confusion of thought, or feeling, which arises generally when others are present. It is distinguished from fear by the fact that it is always caused by persons, whereas fear is an emotion connected with things as well. Nor is timidity a physiological feeling purely, though some of its forms approach this type, thus, for example, the trembling occasioned by the mere presence of an audience or crowd. But even here the emotion depends upon the character of the crowd quite as much as upon its number, the circumstances, etc. The timidity which

seems most purely physical depends largely upon the *ideas* which the crowd evokes in the mind of the individual; that is, the influence which the crowd, as a crowd, exercises is secondary to the feeling which arises from the *thought* of it.

Timidity is not to be regarded as a psychic state of special or determined kind, but is rather a form which affects different states of mind, a sort of malady, or temporary derangement of the *will*, the *intelligence* and the *feelings*. In connection with the will, it is due to the momentary inability to produce certain movements or failure to direct them properly. (*gaucherie*.) All the while the *timide* thus affected is intensely conscious of both the movement he desires to make, and of his inability to execute it. This consciousness constitutes the emotion. This momentary aboulia never attacks the automatic functions of the body. The *gaucherie* seems to be due to the effect produced directly by the *presence* and *regard* of others. The inhibition may not be entire, affecting only the direction of the movements. The inhibiting effect of the presence of others affects the mind, disturbing its functions. M. Dugas calls this form of it *stupidité*. It may be either complete or partial. The first is often taken for lack of intelligence; thus the frequent confusion of pupils in the presence of their teachers. The second type is that of mental confusion, where all direction of thought is lost. There is a total failure of mental adaptation to the occasion or question in hand. On the affective side timidity takes the form of mental stupor. (*stupeur*.) As described by Rousseau and others, this inhibition may be so intense as to cause a complete suspension of the regular intellectual functions, where the subject becomes lost in a purely affective state of pure feeling, or, as in the other two cases, it may be only partial, resulting in a sort of chaos of feeling.

This timidity-feeling is intensely subjective. Not only is the *timide*, *gauche* and *stupide*, he is intensely conscious of it in addition. This is not true in the case of awkwardness and stupidity that arise from ignorance. What then is the relation of timidity to consciousness? Both are due, M. Dugas thinks, to mental incoördination. That is, if adaptation were perfect, we should be reduced to automata, and consciousness would be impossible. But consciousness is the *normal* accompaniment of such mental incoördination, while timidity-feeling is abnormal, being the presence of an *undue* consciousness of this non-adaptation. Timidity-feeling may be of two kinds. It often becomes reflective; more generally, it is spontaneous and involuntary. Thus the falsehoods told by a person in this state

of excitement are not reprehensible, as those told deliberately, since the man speaks before he can reflect. The judgment function is more or less inhibited. All acts and thoughts under the influence of this social inhibition are impulsive, like those of the hypnotic subject.

In its last analysis timidity is found to be due to a lack of sympathetic correspondence between the individual and his social environment. "The subject is not responsive to social magnetism, unable to divest himself of his own peculiar ways of life and thought." In short he is unable to imitate others. He may be intensely conscious of the defect and may feel keenly the need of the sympathy which he fails to exercise and to receive. This lack of responsiveness to social suggestion shows itself in several ways. First, in the attitude of the *timide* toward the *crowd*, further in his treatment of those whom he judges his *superiors*, and lastly in his general unwillingness to *confide* in others. This spontaneous timidity-feeling, which M. Dugas calls *intimidation*, to distinguish it from reflective timidity, is 'due to the distress arising from the realization of the lack of sympathy between ourselves and our environment.' Reflection is apt to create a certain exaggeration of this feeling, so that the person affected 'begins to despise himself, to exaggerate his perplexities, and to pet his anger.' He is apt to isolate himself intellectually. While his thinking may be original, it will lack social adaptiveness. On the affective side there is a tendency to conceal sentiments of his own, and to distrust the sympathy of others. Hence the reserve that is characteristic of timidity. He may be further affected by a certain *maladie d'idéal*, or tendency to despise the things of ordinary life in comparison with his fancies. On the volitional side his acts are apt to be impulsive, and are often incomprehensible to himself, mainly because he no longer has the power of deliberate judgment.

Finally, in its spontaneous form timidity marks a normal state in mental growth, that stands midway between the pure reflex life of the child and reflective mental life. Between the more abnormal form and genius a possible relation is suggested. The exclusiveness which the *timide* seeks, while it cannot of itself inspire art, may give occasion for its development.

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The Popular Aesthetics of Color. JOSEPH JASTROW. Pop. Sci. Monthly, January, 1897. Pp. 361-368.

This is an application of statistical methods to the determination of color preferences. The material for the study—about 4,500 records—

was collected in connection at the Psychological Laboratory of the World's Fair. By means of a convenient system of cards those who were sufficiently interested to stop recorded age, sex, favorite color, and favorite combination of two colors. Twenty-four single colors were displayed from which to choose: red, orange, yellow, green, blue and violet, with six intermediate, and the twelve lighter shades of these. Twenty-four combinations were also shown, presenting as wide a range as possible. The most important as well as the most interesting results are these:

1. The general favorite of all colors is blue, more than one-fourth of the voters choosing this. Red holds the second place, though it is preferred by less than half as many. Then follow lighter blue, blue-violet, red-violet, lighter red (pink) and violet, while the least favorite colors are orange and its shadings toward red and yellow.

2. Darker colors are decidedly preferred to the lighter shades of the same colors, and primary colors (red, orange, etc.) to intermediate (red-orange, orange-yellow, etc.).

3. The difference between the average male and female chooser is striking. The women's favorite color is red, the men's is overwhelmingly blue: "of every thirty masculine votes ten were for blue and three for red; while of every thirty feminine votes four were for blue and five for red." Men confine their choice to relatively fewer colors and have a much less marked tendency than women to choose the lighter and daintier shades.

4. Among the combinations of colors the two most frequently chosen are red with violet, and red with blue; and the most generally avoided are orange with green, violet, or lighter blue. In these combinations the same colors, on the whole, are preferred and avoided as in the single color-preferences.

5. Preference according to age shows (*a*) that blue is least selected by the youngest group (below 18 years), decidedly preferred by the oldest (over forty years), and equally chosen by the groups between these ages; (*b*) that violet is gradually avoided as age increases; (*c*) that lighter red is the preference of the young girls; (*d*) that relatively more persons between twenty-five and thirty than at any other age have 'no choice.'

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PATHOLOGICAL.

Das konträre Geschlechtsgefühl. HAVELOCK ELLIS and J. A. SYMONDS. (Bibliothek für Socialwissenschaft. 7 Band.) Leipzig, Wigand, 1896. xvi + 308.

It would not be right not to enter a protest against the appearance of such a work as this in a library intended primarily for popular reading. Even Krafft Ebing, although writing solely for the medical profession, has been severely and justly criticised for the unnecessary emphasis and importance he has given this subject by his articles on the perversions of the sexual sense, and nothing but harm can follow if popular scientific literature is to suffer a similar deluge. Medical literature of the last few years contains altogether too many histories of these unfortunate individuals who have only discovered themselves to be abnormally afflicted after reading a description of their condition in one of the many monographs or medical journal articles, and the alienist has come to look regularly for a series of sexual pervert autobiographies after the appearance of each new monograph.

If an intelligent understanding of his condition could ever lead to an amelioration of it we might endeavor to endure in silence, but his attention invariably returns to his case and the sexual pervert merely establishes a bond of sympathy between himself and his fellow sufferers; and the world is the worse off in that the sum of morbid introspection has been increased without any corresponding gain whatever. Apart from its influence on the perverts themselves no healthy person can read this literature without a lower opinion of human nature, and this result in itself should bid any writer pause. The writers of the present volume have done their work well, from their point of view, and have threshed over the literature most thoroughly from Bible times down, adding three hundred more pages to a literature already too flourishing.

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Ueber Spaltung der Persönlichkeit. (Sogenanntes Doppel-ich.) VON SCHRENCK-NOTZING. Vienna, Holder. 1896. Pp. 22.

Human personality consists of a complex of elements blending into a unity in the form of self-consciousness; it is, therefore, in constant flux. Besides conscious memory, the seat of which is in the cortex, we must discriminate an organic or hereditary memory (in-

nate reflexes etc.) and a memory for acquired reflexes (walking, etc.) which is probably seated in the basal ganglia. We may also discriminate various grades in consciousness from the clear and focal to the dim and marginal, but, to be conscious at all, a mental state must belong to that one complex, for the word 'conscious' has no other meaning. Féridia X and the other classical cases of successive personality are to be regarded as springing from the addition to, or subtraction from, the sum total of psychic processes which constitute a personality of sundry elements, especially the acquired reflexes. Hence the disorder is manifested in a bewildering variety of forms, the only constant trait being the partial or total destruction of the memory bond between the successive complexes. Pierre Janet's 'geistreiche Auffassung' of hysteria is, in the main, in accord with this view, and it is in no way inconsistent with such a conception of the unity of consciousness as has before been outlined. That *simultaneous* personalities can exist is, however, strenuously to be denied. The cases so interpreted differ from those above described only in this: that two independent thought trains, instead of succeeding one another at long intervals, shift from focus to margin in such rapid succession that the observer is unable to detect any lapse in the movements controlled by each, and, as the memory bond is broken, the patient claims that he is aware of one only, ignoring the other; hence the observer infers a sub-conscious personality to account for the movements which the patient denies producing. Dr. von Schrenck-Notzing adduces no specific evidence in support of this view, but rests his case solely upon the supposed impossibility of admitting that two foci can exist in one and the same organism, or that incoherent, dream-like states may exist out of all relation with any focus.

Telepathy and the Subliminal Self. R. OSGOOD MASON. New York, Henry Holt & Company. Pp. 336.

This book is designed to serve as an introduction to 'Psychical Research' for the use of the general reader. The author writes, on the whole, in a sober vein, the greater number of his cases being very carefully chosen from the publications of the Society of Psychical Research, and his professional position as a physician in good standing will doubtless give his words a weight which they would not otherwise possess. It seems, therefore, of the more importance to call attention to a certain laxity in his sense of the value of evidence, of which illustrations occur more than once and which seriously impairs the value of his book. The earlier series of experiments with the Creery sisters surely cannot

be quoted in proof of telepathy, in view of the fact that the children confessed to the use of signals in the later series. The visions of Elisha, the responses of the Delphic oracle, the marvels told of Pythagoras, the wonders and portents narrated in the pages of ancient historians can have no weight in any cautious mind. More extraordinary still is the statement that Apollonius' vision of the assassination of Domitian rests 'upon the best of ancient authority,' for that same excellent authority, that is, Philostratus' historical romance, narrates, among many other even more extraordinary marvels, how Apollonius detected the plague prowling about Ephesus in the guise of a beggar and caused him to be stoned, whereupon the beggar, dying, changed into a huge black dog of the size of a lion and the plague was stayed. (*Philostr. Vit. Apoll. IV, 10.*) In quoting such cases as evidence, Dr. Mason's zeal seems to have run away with his discretion. This is the more to be regretted because he gives several original observations of phenomena, with reference to which sound evidence is much needed and which would be of great value were their accuracy unquestionable. Especially to be noted is the case on page 71 of the 'magnetization' of water; the one on page 125 of successive personalities and the planchette case on page 159.

By way of explanation the author merely propounds the doctrine of a subliminal self, to which he ascribes all phenomena otherwise inexplicable. Spiritistic conceptions are carefully excluded and the few cases given which would suggest such an interpretation are referred to the agency of the subliminal self telepathically exerted.

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- I fenomeni telepatici e le allucinazioni veridiche.* E. MORSELLI. Florence, Salvatore Landi. 1897. Pp. 58.
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NOTES.

AMERICAN philosophy has been honored by the appointment of Professor Josiah Royce, of Harvard, Gifford Lecturer in the University of Aberdeen for two years beginning 1898-1899.

DR. W. B. PILLSBURY, of Cornell University, has accepted the Instructorship in Psychology and the direction of the psychological laboratory in the University of Michigan.

THE time during which abstracts of papers for the physiological section of the British Association may be sent in has been extended to July 1st. (Dr. A. Kirschmann, Sec., Univ. College, Toronto, Can.)

IN *The Open Court* for May will be found an article on 'The Prophet of Pessimism' by the editor, Dr. Carus, together with a reproduction of the original model of the famous bust of Schopenhauer by Elizabet Ney. This model has been acquired by the Open Court Co., and they offer for sale at the very low price of \$15 a 'limited number' of life-size plaster casts made from it. The undersigned has secured one of these, and finds it admirable in every respect. Philosophers should have it, whatever their attitude toward Schopenhauer; optimists from charity no less than pessimists from loyalty. J. M. B.

A MOVEMENT is on foot to establish a laboratory for experimental psychology with instruction in the subject, in University College, London. A committee, including Francis Galton, E. A. Schäfer and others, are soliciting funds. Professor James Sully is secretary of the committee.

A LECTURSHIP in Physiological and Experimental Psychology has been recommended by the Board of Studies of Cambridge University.

PROFESSOR H. K. WOLFE has resigned the chair of Psychology in the University of Nebraska.

MR. F. C. S. SCHILLER, now at Cornell, has been elected Fellow and Tutor in Philosophy in Corpus Christi College, Oxford.

MR. S. I. FRANZ has been appointed Assistant in Psychology in Columbia University.

W. M. URBAN, PH.D. (Leipzig), has been appointed Reader in Philosophy in the Graduate Department of Princeton University; he will give courses in *Æsthetics*.

PROF. A. C. ARMSTRONG, of Wesleyan University, has been appointed to a chair in History in Princeton University.

DR. W. C. HODGE, of Princeton, has been appointed Associate Professor of Philosophy in Lafayette College.

DR. C. E. SEASHORE, of the Yale Psychological Laboratory, has been made Assistant Professor in the University of Iowa.

E. M. WEYER, of the University of Leipzig, and M. Matsumoto, of the University of Tokio, Japan, have been appointed assistants in the Yale Psychological Laboratory.

THE prospectus has been issued of a new *Revista Italiana di Sociologia*, edited by a board on which Professor G. Sergi will represent psychology. The address is: Rome, 42 Piazza Poli.

